THE GENERAL CRISIS OF THE SEVENTEENTH CENTURY

Second edition

EDITED BY
GEOFFREY PARKER
AND LESLEY M. SMITH

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THE GENERAL CRISIS OF THE SEVENTEENTH CENTURY

One of the fiercest and most wide-ranging debates in historical circles over the last forty years has concerned the theory that, throughout Europe, the seventeenth century saw a period of crisis so pervasive, significant and intense that it could be labelled a 'General Crisis'. A number of articles stimulated by the debate were collected and published in *Crisis in Europe*, edited by Trevor Aston (Routledge & Kegan Paul, 1965). There followed a new collection of essays—containing fresh research and new perspectives— *The General Crisis of the Seventeenth Century*, edited by Geoffrey Parker and Lesley Smith (Routledge & Kegan Paul, 1978).

This revised edition of *The General Crisis* takes the still undecided debate up to the present day. The editors have provided a new introduction, updated several of the earlier essays and added four new chapters concerning the social, economic and political crises that affected not only Europe, but also Asia, in the mid-seventeenth century. These pieces form essential reading for a clear understanding of the period.

Editors: Geoffrey Parker, The Ohio State University, and **Lesley M.Smith,** George Mason University, Virginia.

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For Margaret and Gael

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PREFACE

The ‘General Crisis theory’ has been with us now for forty years and shows no sign of dying. In 1965 the late T.S.Aston, editor of *Past and Present,* published a volume of thirteen essays which had previously appeared in that journal and illuminated one or more aspects of the seventeenth-century crisis. That volume, published like this one by Routledge, was entitled *Crisis in Europe, 1560–1660.* Since 1965 many more articles and essays have appeared on the subject, often in foreign journals, and from these we have selected ten of the most substantial, five of them published abroad. In many ways this volume is a sequel to *Crisis in Europe* since, although none of the material in that collection is reproduced here, most of our contributors review and refine the opinions advanced in the earlier work. The two books should be seen as complementary.

In revising this collection of essays, first published in 1978, we have added four new items—one on Germany and the other three on East Asia. We have also thoroughly updated our introduction to take account of these and other new studies of the General Crisis. The remaining six contributions have been left in their original form, although Ruggiero Romano has provided a ‘postscript’.

The editors still remember fondly their discussions with André Carus, James Coonan, Stephen Davies, Andrew Gailey and Catherine Greenhalgh at St Andrew’s University, as they prepared the first edition of this book, and also the expert assistance of Andrew Wheatcroft in preparing it for the press. They wish to repeat their gratitude to the authors and publishers of the contributions included in this book for permission to reprint the articles. And, finally, they renew the dedication to Margaret Wallis and Gael Newby, their friends for twenty-five years.

1
INTRODUCTION[1]

Geoffrey Parker and Lesley M.Smith

I
HISTORIANS AND THE 'GENERAL CRISIS'

In 1642 John Goodwin, a pamphleteer, predicted that 'the heate and warmth and living influence' of the English House of Commons in their struggle with King Charles I 'shall pierce through many kingdoms greate and large, as France, Germany, Bohemia, Hungaria, Polonia, Denmarke, Sweden and many others'. One year later Jeremiah Whittaker, a preacher, reassured the same assembly that it did indeed not stand alone in rebellion: 'These are days of shaking and this shaking is universal: the Palatinate, Bohemia, Germania, Catalonia, Portugal, Ireland, England.' In 1654, with the king executed and all his adherents defeated, John Milton imagined 'that from the columns of Hercules to the farthest borders of India, I am bringing back, bringing home to every nation, liberty so long driven out, so long an exile'.[2] Interest in international affairs by no means remained confined to those at the centre of the English Revolution. Between 1652 and 1664 Ralph Josselin, the vicar of the village of Earls Colne in Essex, often sat down on the eve of his birthday to record the 'great actions' of the previous year in both Britain and Europe. His survey on 25–6 January 1652 (to take but one example) reveals the speed with which the tremors of revolution abroad reached even a small community. Having summarized Cromwell's successful campaigns in England, Scotland and Ireland, Josselin continued:

> France is likely to fall into flames by her owne divisions; this summer shee hath done nothing abroad. The Spaniard hath almost reduced Barcelona, the chiefe city of Catalonia, and so that kingdome; the issue of that affair wee waite. Poland is free from warre with the Cossacks but feareth them. Dane and Suede are both quiet, and so is Germany, yett the peace at Munster is not fully executed: the Turke hath done no great matter on the Venetian, nor beene so fortunate and martial as formerly, as if that people were at their height and declining rather.[3]

Foreign observers showed equal awareness of the unusually widespread political instability of the mid-seventeenth century. Writing early in 1643 some friends of the count-duke of Olivares, the disgraced chief minister of Philip IV of Spain, noted a global context for the failures of their hero in the apologetic tract, *Nicandro:*

> All the north in commotion… England, Ireland and Scotland aflame with Civil War… The Ottomans tearing each other to pieces… China invaded by the Tartars, Ethiopia by the Turks, and the Indian kings who live scattered through the region between the Ganges and the Indus all at each other's throats.[4]

A few years later, a Scottish exile in France, Robert Mentet de Salmonet, prefaced his account of the English Revolution with the general statement that he and his fellow Europeans were living in an 'Iron Age' that would become 'famous for the great and strange revolutions that have happen'd in it… Revolts have been frequent both in East and West.'[5] Others attempted explicit comparisons: in 1642–3 the Czech Wenceslaus Hollar engraved 'A comparison of the English and Bohemian Civil Wars' (see Figure 1.1); in 1653 the Italian Giovanni Battista Birago Avogadro published a volume of studies that covered the uprisings in Catalonia, Portugal, Sicily, England, France, Naples and Brazil (based on information drawn from the reports published in newspapers such as the *Mercure François*); while in 1669 the Dutchman Lieuwe van Aitzema drew parallels between the Naples uprising of 1647 and the Moscow revolt of 1648.[6] By the 1660s, however, Thomas Hobbes could already see that the mid-century troubles, although memorable, remained isolated. His *Behemoth,* a personal account of the civil war, began with the words, 'If in time, as in place, there were degrees of high and low, I verily believe that the highest time would be that which passed between the years l640 and 1660.'[7]

Voltaire, the philosopher, writer and all-purpose luminary of the French Enlightenment, was apparently the first person to see the events of mid-seventeenth-century Britain and Europe as part of a global crisis. In 1741–2 he composed a history book, *Essai sur les mœurs et l'esprit des nations,* for his friend, Mme du Châtelet, who was bored stiff by the past. The seventeenth century, with its numerous revolts, wars and rebellions, presented Mme du Châtelet with special problems of *ennui* and, in an attempt to render such anarchy more palatable, Voltaire advanced a theory of 'general crisis'. Having grimly itemized the political upheavals in Poland, Russia, France, England, Spain and Germany, he turned to the Ottoman Empire where Sultan Ibrahim was deposed in 1648. By a strange coincidence, he reflected:

> This unfortunate time for Ibrahim was unfortunate for all monarchs. The crown of the Holy Roman Empire was unsettled by the famous Thirty Years' War. Civil war devastated France and forced the mother of Louis

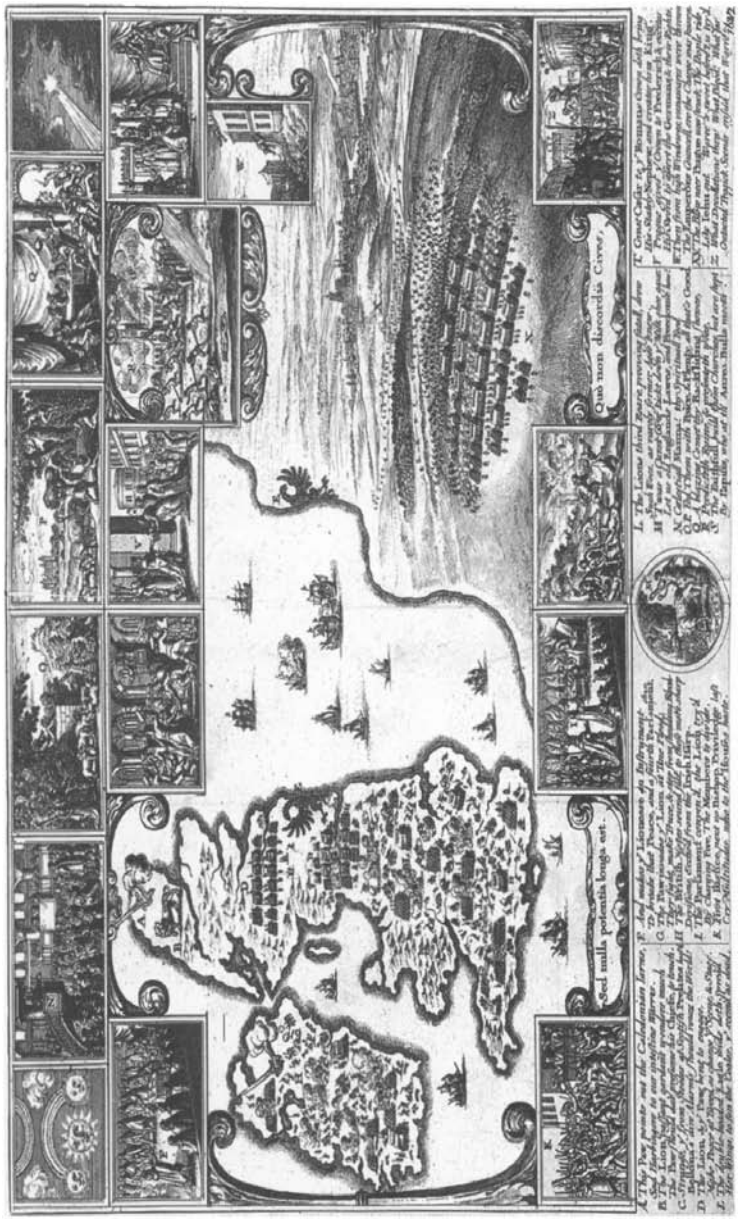

Figure 1.1 A comparison of the English and Bohemian Civil Wars
Source: R.T.Godfrey, Wenceslaus Hollar: A Bohemian Artist in England (New Haven, CT, 1994), p. 92.
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XIV to flee with her children from her capital. In London Charles I was condemned to death by his own subjects. Philip IV, king of Spain, having

> lost almost all his possessions in Asia, also lost Portugal.

The mid-seventeenth century, Voltaire concluded, constituted 'a period of usurpations almost from one end of the world to the other'; and he pointed not only to Cromwell in Europe but also to Muley-Ismail in Morocco, Aurangzeb in India and Li Tsu-Ch'eng in China.[8]

This 'world dimension' lay at the centre of Voltaire's whole *Essai*, and he returned to it later on, positing an almost organic theory of change.

> In the flood of revolutions which we have seen from one end of the universe to the other, there seems a fatal sequence of events which dragged people into them, just as winds move the sand and the waves. The developments in Japan offer another example.

And so he continued, writing one of the earliest—and most readable—genuinely global histories ever composed.[9]

Voltaire perceived an important feature of the General Crisis that many subsequent writers have forgotten: the phenomenon did not remain confined to Europe.[10] On the whole, his twentieth-century successors have proved less inclusive. Paul Hazard portrayed this period as one of fundamental change in his 1935 classic *La Crise de la conscience européenne*, but restricted his attention to Europe. Three years later the concept passed from the history of ideas to politics and economics in Roger B.Merriman's *Six Contemporaneous Revolutions* (Oxford, 1938) but again only the major European upheavals of the 1640s featured. In 1954, E.J.Hobsbawm's long article, 'The crisis of the seventeenth century' (*Past and Present*, V and VI, 1954), reviewed the problems of the European economy and postulated the existence of a 'general crisis' there too. However, Woodrow W.Borah's *New Spain's Century of Depression* (Berkeley, 1951) examined the economic crisis in seventeenth-century Mexico, and related it to developments in Europe; while Roland Mousnier's *Les XVIe and XVIIe siècles* (Paris, 1954) characterized the history of the entire world between 1598 and 1715 as a 'century of crisis', with demographic, economic, political, diplomatic and intellectual manifestations affecting all the major civilizations.[11]

Hobsbawm's essay stimulated a host of equally Eurocentric articles in *Past and Present*, most of them republished in T.S.Aston (ed.), *Crisis in Europe, 1560–1660* (London, 1965). Then came a challenging survey by T.K.Rabb, *The Struggle for Stability in Early Modern Europe* (Oxford, 1976); a special issue of the journal *Revue d'histoire diplomatique* (Paris, 1978), containing eleven case studies; and an issue of the journal *Renaissance and Modern Studies* (Nottingham, 1982) on '"Crisis" in Early Modern Europe', with eight more articles. Finally, at the 1989 meeting of the American Association for Asian Studies, four papers on 'The General Crisis in East Asia', published in 1990 in *Modern Asian Studies*, belatedly restored the world dimension to the General

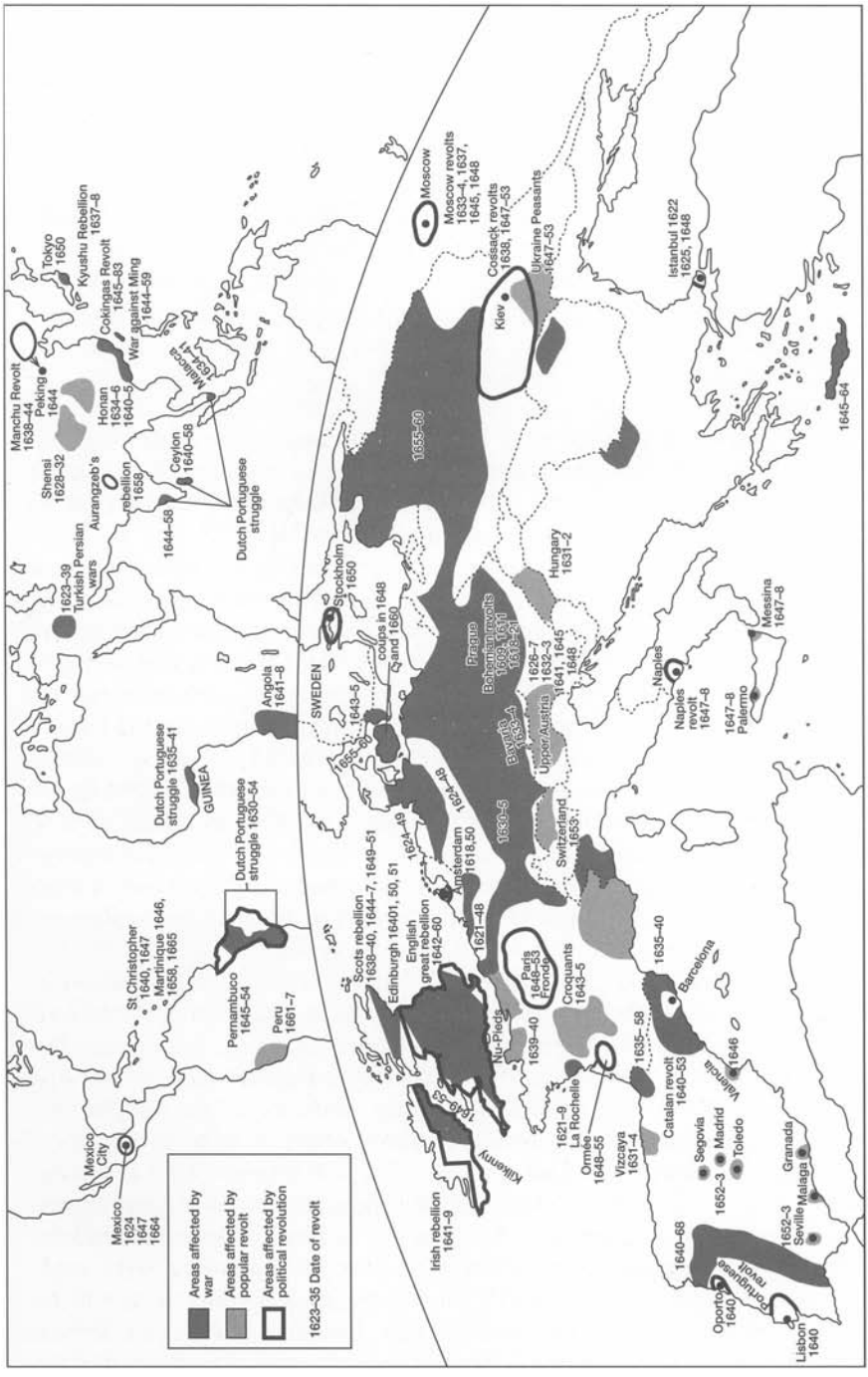

Figure 1.2 The 'General Crisis'

Crisis debate.[12] For, as Figure 1.2 shows, upheavals and rebellions occurred in almost all areas of the Old World, and also in some parts of the New.

In fact, simultaneous global unrest had happened before. About the middle of the second millennium BC several pre-classical civilizations of Eurasia (Minoan Crete, Mycenaean Greece, the Egyptian Middle Kingdom) collapsed; in the fourth and fifth centuries AD the main classical civilizations foundered almost at the same time (the Chin, Gupta and Roman empires); and in the fourteenth century, even before the Black Death, the leading states of the High Middle Ages either tottered or crumbled (the Yüan in China, the Minamoto in Japan, the Ghazni in India, the feudal monarchies in Europe).[13] At least one more such 'general crisis' took place after the seventeenth century: between 1810 and 1850 (see p. 22).

This pattern of world-wide upheaval suggests that beneath the more obvious local precipitants lurked very basic global causes. The most plausible candidate is climatic change: a world-wide deterioration in prevailing weather patterns, leading to relative over-population and related food shortages, to mass migrations (perhaps armed) from poorer to richer lands, to swiftspreading pandemics, and to frequent wars and rebellions. However, for a climatic explanation to be valid, evidence of crisis must be apparent in many parts of the globe—at least over large parts of the northern hemisphere (where the bulk of the world's population then lived)—and not merely in a small region such as Europe. The mid-seventeenth century seems to meet this criterion. The peasant revolts in Ming China between 1628 and 1644 resembled the simultaneous wave of popular uprisings in France under Richelieu; increasing military expenditure and court extravagance at a time of economic adversity characterized both societies; and both in Peking in 1644 and in Paris in 1648 'usurpers' (to use Voltaire's phrase) eliminated a sovereign of undisputed legitimacy. Disastrous plague epidemics also ravaged both countries—1639–44 in China; 1630–2 and 1647–9 in France—and numerous sources across Eurasia recorded climatic deterioration.[14]

Nevertheless, the transition from world economic crisis to individual political upheavals depended upon personal decisions, local conditions and unforeseen accidents to a degree that makes generalization hazardous. The General Crisis in fact comprised two related elements: on the one hand, a major hiatus in the demographic and economic evolution of the world which increased the probability that political tensions would escalate into violence; on the other, a series of political crises, some of which developed into revolutions while others did not. Since the links between the two were neither simple nor obvious, it is prudent to remember the advice of John Selden, the seventeenth-century wit, cynic and antiquarian, to those who were overzealous in bringing witches to trial: The reason of a thing is not to be enquired after,' he warned, ''til you are sure the thing itself is so. We are commonly at *what's the reason of it?* before we are sure of the thing.'[15] In the case of the General Crisis, to ensure that we have

understood the 'thing' aright, it is prudent to study the economic and political developments separately before examining the links between them.

II
AN ECONOMIC AND SOCIAL CRISIS

Most historians know about the 'Little Ice Age', once parochially described in English school textbooks as 'the time the Thames froze'. The problem has always been to explain it. The work of an American astronomer, John Eddy, on the scarcity of sunspots during the late seventeenth century (the so-called 'Maunder Minimum') suggests an important new explanation for the phenomenon (see Chapter 11). The leading astronomers of the late seventeenth century—including Johan Hevelius (1611–87) in Poland, G.D.Cassini (1646–1712) in France and John Flamsteed (1646–1719) in England—all recorded an almost complete absence of sunspots between about 1645 and 1715, while noting that their predecessors Galileo and Scheiner had observed them earlier in the century. These skilful European astronomers also failed to see either aurora borealis (northern lights) or a corona during solar eclipses during this period. Such data cannot be dismissed as absence of evidence: they constitute genuine evidence of absence. When Flamsteed, the Astronomer Royal, saw a sunspot in 1684, he wrote a learned paper on the subject because he had searched in vain for one during the previous eight years (see p. 268). Recent research on glacier movements and harvest dates in Europe has shown that harvests in the mid-seventeenth century occurred far later than normal, and suggests long winters and excessive rain as the principal culprits.[16] Moreover, measurements of the radioactive isotope of carbon, carbon–14 (^{14}C), present in tree-rings reveal a striking aberration: at precisely this time an enormous increase in ^{14}C deposits occurred, indicating an abundance of carbon in the earth's atmosphere, a circumstance normally associated with a reduction in solar energy. Some suspected a correlation between sunspots and climate even during the crisis years of the 1640s. European observers noted the coincidence of sharp fluctuations in the number of sunspots and unusually harsh climatic conditions and suggested that few sunspots might create drought, while others believed that many sunspots would drive down global temperatures.[17]

Other factors may also have reduced the heat of the sun received on earth at this time. Contemporaries who studied weather patterns in early modern Korea also noted the relative scarcity of sunspots and the lack of a corona but concentrated more on the high incidence of atmospheric dust. A recent study suggests that the abnormal frequency of meteor showers recorded in seventeenth-century Korean sources (1305 meteors, as against only 716 in the sixteenth century and 898 in the eighteenth century) generated dust clouds that reduced the solar energy reaching the earth. The dramatic increase in volcanic activity during these years would also have intensified this phenomenon. Whatever the cause, the Royal Annals kept in Seoul repeatedly noted during the mid-seventeenth century that

'from morning to evening the skies all around have been darkened and gray as if some kind of dust has fallen', and that 'We are experiencing a severe drought.'[18]

William S.Atwell and Anthony Reid agree that a deteriorating global climate, however caused, adversely affected food resources elsewhere in East Asia—both north and south of the equator—during the mid-seventeenth century (Chapters 8 and 9). In particular, the remarkable series of tree-rings from the teak forests of Java reveal prolonged periods of drought on the whole island in 1605–16, 1633–8 and, above all, 1643–71 (see Figure 8.1). Chinese and Vietnamese sources also recorded droughts in the 1640s; in Japan, repeated harvest failures and climatic disasters caused a severe famine during the late 1630s and 1640s ('The Great Famine of the Kan'ei Period'), while India, Siam and Burma suffered cruelly in 1630–3 and again in 1660–1.[19] These natural disasters resulted, predictably, in high food prices (although they did not always produce famine in areas like Indonesia, with abundant alternative supplies of food) and in epidemic disease, the two combining to produce severe mortality crises. Southwest Africa, too, followed much the same pattern, with marked periods of drought during the late sixteenth and mid-seventeenth centuries leading to scarcity of food, heightened mortality, increased conflict and enhanced migration.[20]

In a world dependent largely on vegetable and cereal crops, such correlations should cause no surprise. Between 80 and 95 per cent of the early modern population depended directly on crop yields. According to Fernand Braudel:

> It is generally admitted that the annual consumption per head of wheat (and other cereals) was of the order of two (present-day) quintals… If the population [of the Mediterranean area] was 60 million, the total annual consumption of wheat or other bread crops must have been about 120 million quintals. Other foodstuffs, meat, fish, olive oil, and wine were merely complementary to the staple diet. If we take the average price of the quintal in about 1600 to be 4 or 5 Venetian ducats, Mediterranean consumption (assumed equal to production) must have reached 480 or 600 million ducats every year… Grain alone establishes the overwhelming superiority of agricultural production over all others.[21]

A fall of 1 degree Celsius in overall temperatures—and that appears to have been the magnitude of the change during the 'Little Ice Age'—restricts the growing season for plants by three or four weeks and reduces the maximum altitude for the successful cultivation of foodstuffs by about 500 feet. In some sensitive or marginal areas the impact could be even greater: in Iceland today, a 1 degree fall in overall summer temperature reduces crop yields by 15 per cent.[22] The expansion of population in the sixteenth century had led to the cultivation of many marginal lands; a run of colder summers in the seventeenth century would have reduced or perhaps extinguished the yield of such areas, leaving their populations on the threshold of starvation.[23] Furthermore, diminished food reserves, producing (in effect) serious overpopulation, presented a favourable

terrain for the spread of diseases, which removed the weaker members of communities that had outgrown their resources. A skilful reconstruction of disease patterns in London between 1670 and 1830 suggests a dual mechanism: the ravages of epidemic diseases such as bubonic plague kept pace with food prices, while the impact of endemic diseases such as dysentery varied with temperature.[24]

Economists have termed this dilemma a 'high-level equilibrium trap'. The inputs and outputs of the early modern agrarian system had reached a balance that could be broken only by heavy capital investment and new technology, and European agriculture lacked both. As Niels Steensgaard notes on pp. 35–6, yield ratios during this period remained stable or fell. In eastern and central Europe they stood at four grains of corn harvested for every grain sown, which was scarcely enough to feed those who produced it; yields in Atlantic Europe rose somewhat higher, but until 1700 they still provided an insufficient basis for economic diversification. And wherever yields did improve, whether through better methods or increased area of cultivation, population growth soon swallowed up surplus production, short-circuiting the process of capital accumulation necessary for technical innovation or agricultural improvement. A population caught in this trap faced only three choices: migration, starvation or revolt.[25]

Even in 'ordinary' years, from the later sixteenth century onwards population drifted from smaller villages towards towns and cities. In the Montes region, south of Toledo in central Spain, 'Smaller villages located in higher, less fertile regions became overpopulated at an early date, with a consequent migration to larger settlements.' The grain harvest records for the area reveal a dramatic, sustained fall in yields from 1615 onwards, and entire hamlets in the uplands were abandoned.[26] Many of the 'lost' inhabitants migrated to the cities, especially to Madrid, which increased in size from 65,000 people in 1597 to 140,000 in 1646, thanks largely to the influx of almost 5,000 migrants per year. But Madrid constituted one of seventeenth-century Spain's few success stories; most other towns failed to increase and, in some cases, the growth of one town involved the decline of others—in particular, Madrid gained at the expense of Toledo. The urban history of England in the seventeenth century differed little. Perhaps 10 per cent of the English population lived in towns in 1500 and perhaps 20 per cent in 1700, but London alone, which grew from 25,000 to 575,000 people during this period, accounted for over half of this increase.[27] In effect, these economic migrants constituted a permanently mobile population: in the small Essex town of Cogenhoe between 1618 and 1628, 52 per cent of the population changed. Much the same turnover rate characterized the much larger port city of Southampton.[28] The literature of the period bristles with fear of these migrants, and an awareness of their growing numbers. Stories about engaging rogues such as *Till Eulenspiegel* or *Lazarillo de Tormes*—like the more scientific *Vocabulario de Germania* of 1609, which listed 1,300 special words used by Spanish beggars, and *Il Vagabondo*, an encyclopaedic compilation of the

customs and life of the Italian underworld, published by Giacinto Nobile in 1627—all exuded an undercurrent of menace and instability.[29]

However, not all the 'missing' population packed their knapsacks and took to the roads. Many died in plague epidemics, which could remove between 20 and 30 per cent of a region's population at a stroke; many others were simply not born, for during the mid-seventeenth century birth rates stagnated and even declined. Contemporaries first noted this phenomenon in Castile, where the population may have fallen by 50 per cent between 1600 and 1650. In 1619 a puzzled canon of Toledo Cathedral, Sancho de Moncada, first employed 'modern' methods to measure depopulation; while consulting the parish registers of the region he found 'in the years 1617 and 1618 not one half of the marriages that there used to be'. This, he felt, explained why there were fewer children. Moncada went on to suggest that the primary reason for the persistent fall in marriages, and therefore in population, lay not in migration, plague or the expulsion of the Moriscos (as other authors had argued), but 'because the people cannot support themselves'. He postulated a clear correlation between population decline and poor harvests, for poor harvests caused the price of essential items—above all bread—to increase beyond the reach of the average wage-earner.[30]

Subsequent research has totally vindicated this pioneer of historical demography. In almost every community of early modern Europe where historians have studied the complete data, the unpredictable yet irresistible rhythm of bread prices appears to have controlled the level of marriages, conceptions and deaths: whenever bread prices and deaths rose, marriages, conceptions and therefore births all fell. The experience of Baugé in Anjou (France) between 1691 and 1695 (see Figure 1.3) offers a typical example of the demographic consequences of the 'subsistence crises' that apparently struck most European communities at least once per generation during the early modern period.[31] But the frequency of crises could sometimes increase dramatically Significantly, no less than three occurred in the mid-seventeenth century: one in 1643–4, a second (the worst of the entire century) in 1649–50, and a third in 1652–3. These harvest failures affected all Europe, from Poland to England and from Sweden to Italy (see Figure 1.4).[32]

In many cases harvest failure also precipitated industrial and commercial crises, for the sharp rise in food prices led to a falling demand for manufactured goods, which in turn led to widespread unemployment among wageearners. Many families therefore lost their main source of income just as the price of essential items escalated. Niels Steensgaard (Chapter 2), Sheilagh Ogilvie (Chapter 3) and Ruggiero Romano (Chapter 7) all agree that these recessions became particularly common during the seventeenth century. Romano, writing originally in 1962, saw the crisis of 1619–22 as the decisive break, since in its wake international trade, industrial output, silver imports from America, and coinage issues all fell. Recovery was inhibited, he argued, by a crisis in agriculture, where tillage had lost ground to stock-raising and refeudalization had spread (especially in eastern Europe.) Steensgaard, however, writing in 1970,

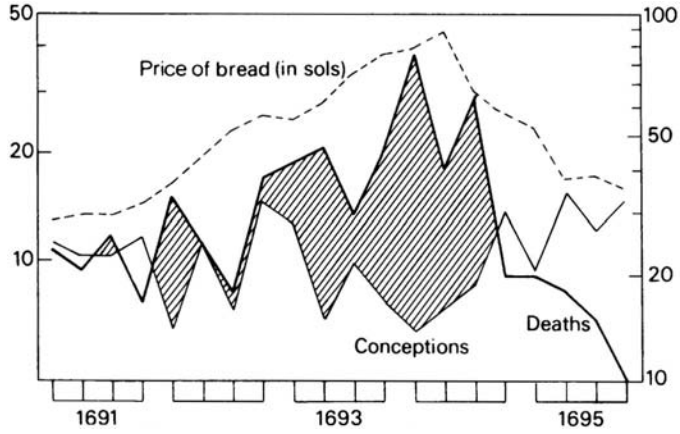

Figure 1.3 A subsistence crisis in Anjou, 1691–5

perceived a problem of distribution, rather than of production, caused by the enormous growth in the public sector. Government spending rapidly increased, causing the diversion of economic endeavour to meet the demands of the public sector through the transfer of resources to the state via heavy taxation. Ogilvie, in 1992, demonstrated that neither model entirely fits Germany—an area omitted from most accounts of the General Crisis[33]—where regional diversity makes any generalization hazardous. However, her data largely support Steensgaard's argument that the growth of taxation to finance armies put pressure on economies lacking large surpluses, causing both widespread suffering and, in many cases, in Germany as elsewhere, rebellion.

'The peasant revolt,' wrote Marc Bloch, 'was as common in early modern Europe as strikes are in industrial societies today.' Astonishing numbers of rural uprisings took place in certain areas: Provence, for example, saw 108 popular rebellions between 1596 and 1635, 156 between 1635 and 1660 (16 of them associated with 'Fronde' of 1648–53) and a further 110 between 1661 and 1715. For a region of barely 600,000 people, a grand total of 374 revolts over scarcely more than a century is impressive![34] Certain German and Dutch towns also experienced numerous uprisings in the seventeenth century.[35] However, an important difference distinguished popular revolts from strikes: the latter aimed to influence the employer, landlord or owner for whom the strikers worked, while the early modern revolt was directed overwhelmingly against the state, particularly during the period 1625–75. Neither the exactions of the Church and nobles, nor their exemption (in many countries) from taxation, seems to have triggered a widespread revolt in the West at this time.[36] Such burdens were normally regarded in the same light as freak weather or a bad harvest—unpleasant, but immutable and inescapable. Rather, rebels tended to target grievances that could, in theory, be redressed —the policies and demands of

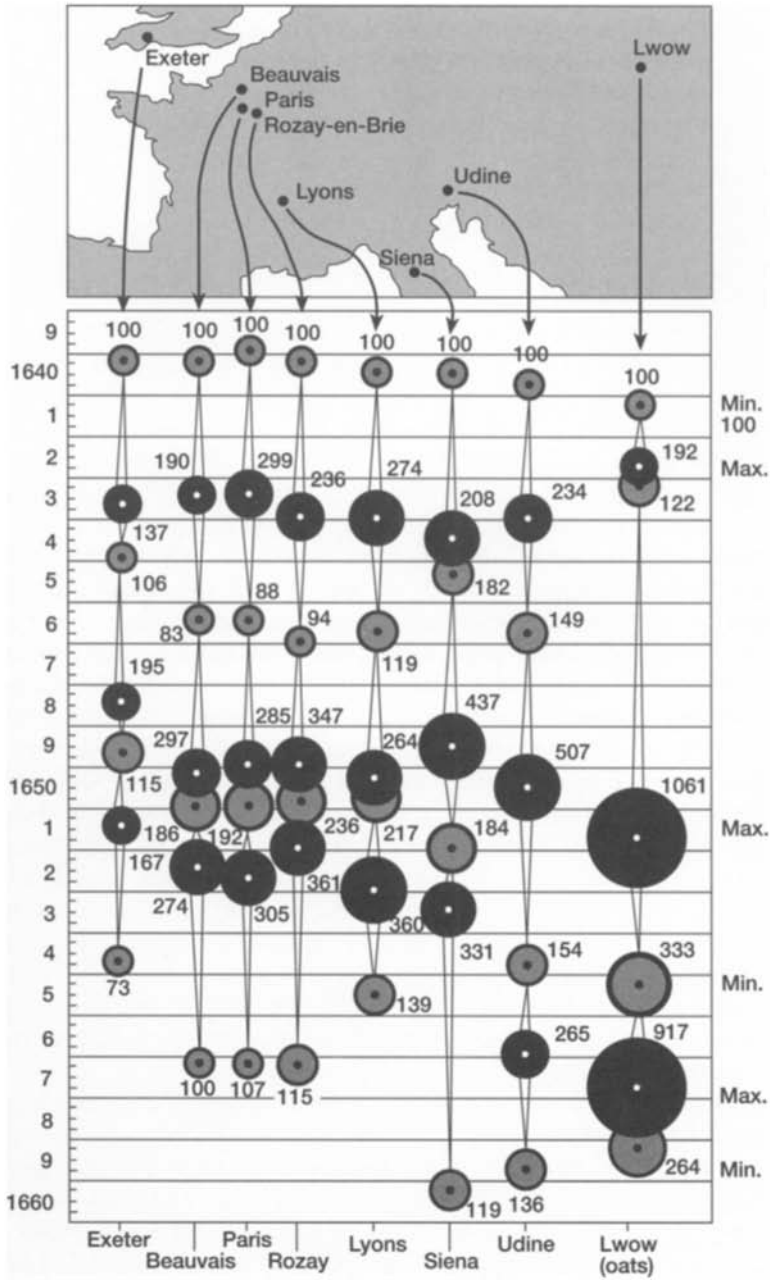

Figure 1.4 Subsistence crises in Europe, 1639–60

Note: The base period from fourth quarter 1639 to 1641=100

Source: E.E.Rich and C.H.Wilson (eds) *The Cambridge Economic History*, IV (1967), p. 468.

government—and the commonest victims of the rioters were normally the officials who tried to enforce those policies, especially tax-collectors.

The pattern of each of these rebellions reflected a tradition of popular uprisings that went back at least to the fourteenth century and continued down to the late eighteenth century, and perhaps beyond. Even the actions of the crowd remained the same, as if the oral culture of a community kept alive the traditions of 'proper behaviour' during a riot. Thus the crowd that rallied to the *'stendardo rosso',* the red flag raised above the city of Naples in 1647, did much the same things, in much the same ways, as their predecessors during the risings of 1510, 1548 and 1585. Their descendants would follow suit during the rising of 1799.[37] Rural areas, too, could serve as semi-permanent oases of revolt. The estates of the Schaunberg family lay at the heart of each one of the Upper Austrian peasant revolts in 1511–14, 1525, 1560, 1570, 1595–7, 1620, 1626, 1632–3 and 1648. In Aquitaine in south-western France, few major popular uprisings in the seventeenth century took place without the participation of men from the *vicomté* of Turenne, the heathland estates of Angoumois or the marshlands around Riez. Men from the Cornish parish of St Keverne in south-western England participated in the revolts of 1497, 1537, 1548 and 1648.[38]

III
A POLITICAL CRISIS

But what, precisely, linked this economic and social crisis with the simultaneous political upheavals? The immediate and deleterious impact of war and rebellion upon economic activity can be easily demonstrated but evidence of economic malaise and social unrest precipitating a political crisis seems more elusive. Nevertheless, a letter written in October 1637 by the Earl of Lothian in Scotland to his father, then at the court of Charles I in England, graphically revealed the political dangers inherent in adverse economic circumstances. State tax demands had soared from £200,000 Scots in 1600–9 to £4,000,000 in 1630–9, while trade atrophied and harvests failed, plunging many landowners deep into debt. According to Lord Lothian:

> The earth hath beane iron in this land (especially in Lothian), and the heavens brass this summer, till nowe in the harvest there hath beane sutch innundations and floodes and wyndes, as noe man livinge remmembers the like. This hath shaken and rotten and caried away the litle corne [that] came up, [so] that certainly they [who] are not blynd may see a judgement come on this land. Besides there is noe kynd of coyne in 't, [so] that men [who] are in debt can not gett there owne to give there creditors, and the few that have money keepe it for themselves for the greate advantage in this penurie and necessitie. So that, for me, if your Lordshipp perswade not my Lord Tresorer to pay the arrearages of your pension, I thinke I shal be

> forced this terme to runn away and let the creditors catch may [= me], for I cannot doe impossibilities.

But the earl did not 'runn away'. Before the winter ended, he had signed the National Covenant and thrown in his lot with the opponents of the king. As Keith Brown has shrewdly observed, these and other similar exclamations of hardship do not mean that the Scottish Revolution was all about money. Lord Lothian and others like him also felt genuine outrage towards Charles I's ecclesiastical policy and high taxes, but, in the final analysis, their 'minds were made up for them during the harsh winter of 1637–8 following a long hard look at their accounts'.[39]

Fiscal pressure at a time of economic crisis also helps to explain the frequency of revolt elsewhere. In the *généralité* of Bordeaux, France, for example, the *taille* (a direct tax) remained constant from 1610 to 1632, at one million livres; but in 1635, when France went to war against Spain, it rose to two million livres; in 1644, as grain prices began to soar, it rose to three million livres; and in 1648, coinciding with the worst harvest of the century, it rose to four million livres. Small wonder that the area became a centre of rebellion during the Fronde later that year.[40] Many other European political crises in these years exhibited a similar pattern: in the various restless realms of Philip IV, in Holland, in Sweden and in Muscovy, governments sought to impose a plethora of innovative policies, especially in the field of taxation, upon a social structure and an economy already under stress.

In Chapters 2 and 10, Niels Steensgaard highlights the marked rise in government activity and expenditure that took place in the seventeenth century, most of it caused by war, preparation for war, or liquidating the consequences of war. For, as the state's revenues increased, so its armed forces grew; and each increase naturally provoked further tax demands (see Table 1.1).[41]

Moreover, price inflation meant that the cost of putting each of these soldiers in the field rose by a factor of five between 1530 and 1630. The cost of waging war thus reached a level that few governments could long afford.[42] On the one hand, as noted above, taxes to pay for the armies (and, in maritime states, also the navies) increased dramatically; on the other, dislocation and disruption grew alarmingly. Thus in November 1643 the Governors of the English East India Company, writing from London to their agents in India, attributed their failure to export the normal quantity of goods entirely to the wars then ravaging not only the kingdoms ruled by Charles I, but also much of the continent.

> Wee are fearful how far wee shall be able to performe in this trouble-some tyme, when all trade and commerce in this kingdome is almost fallen to the ground through our owne unhappie divisions at home, unto which the Lord in mercie put a good end. And as the badnesse of trade and scarsity of mony are here, so is all Europe in little better condition, but in a turmoyle,

> either forraighne or domestique warr, by which meanes monies are not procurable as formerly.[43]

Rising costs did not limit hostilities, however. On the contrary, armed conflict proved more common in Europe during the seventeenth century than ever before: only four complete calendar years saw peace over the whole continent. Some states, most notably Spain and her dominions, spent almost the entire century engaged in war; and hostilities, once commenced, tended to involve several states.[44] Even in peacetime, most large European states aspired to a standing army, which required payment whether it saw active service or not. However, as Ogilvie points out (p. 68):

> The growth of the state in early modern Europe affected more than taxation and warfare. The administrative instruments developed for *these* purposes could also regulate activities previously inaccessible to government, and they could offer redistributive services to a wide range of favoured groups and institutions. Resistance to these *new* forms of redistribution, and competition to control them, were central elements in the crisis.

Table 1.1 Increases in military manpower, 1490–1710

Date	*Spanish Monarchy*	*Dutch Republic*	*France*	*England*	*Sweden*	*Russia*
1490s	80,000		40,000			
1550s	150,000		70,000	20,000		
1590s	150,000	20,000	60,000	30,000	15,000	
1630s	150,000	70,000	125,000		125,000	35,000
1650s	100,000		100,000	70,000	70,000	
1670s	70,000	110,000	253,000		63,000	200,000
1700s	50,000	100,000	255,000	87,000	90,000	220,000

And, indeed, Steensgaard (Chapters 2 and 10), Schöffer (Chapter 4) and Elliott (Chapter 5) all stress that behind every major political revolt of the period lay resentment against government attempts to change the *status quo*. In every case the 'court', not the 'country', appeared as the innovator.[45] In the words of Niels Steensgaard, 'The six contemporaneous revolutions can only be seen as one if we rechristen them the "six contemporaneous reactions"' (p. 47). Thus in Britain, the initiative of the Crown—whether in matters fiscal (Ship Money), diplomatic (alliance with Spain), economic (new monopolies) or religious (Arminianism)—repeatedly shook the *status quo* and provoked discontent. Even the culture of the Caroline court was new. Some claimed that plays, 'masques', romantic poetry and unrestrained dancing had 'poisoned the old manners of our Country with foreign delights'. The court had become intellectually as well as politically isolated.[46] In France, Spain and the Netherlands, too, the court's

devotion to stage plays and the theatre provoked widespread criticism (exacerbated in Holland by a linguistic grievance: the House of Orange's players were French and performed in The Hague in French).

Religious differences also increased the cultural isolation of the courts in central Europe. In the Holy Roman Empire, the Peace of Augsburg of 1555 had granted each secular prince the right to choose his own religion and enforce it on his subjects; the Confederation of Warsaw of 1573 introduced a similar scheme in Poland-Lithuania. This arrangement placed great power in the hands of territorial rulers, for their religious policy dictated the creed of everyone else, and it ushered in a period of confessional absolutism. In the Polish Commonwealth, the lord of Rákow enforced Socinianism on his tenants, while in the royal domains of Mazovia the crown enforced Catholicism. Magnates implementing the *cuius regio eius religio* principle shut down perhaps two-thirds of Poland's 760 Protestant churches between 1606 and 1620. In the Empire, the Rhine Palatinate changed from Catholic to Lutheran in 1544, from Lutheran to Calvinist in 1559, back to Lutheran in 1576 and back to Calvinist in 1583 simply because its successive rulers professed different religions. In the Habsburg lands, the Jesuit-trained Ferdinand II decreed the expulsion of all Protestants from Styria when he came to power there in 1600; from Bohemia, after he had defeated his rival for the crown, Frederick of the Palatinate, in 1620; and from Upper Austria with a special *Reformationskommission* in 1625. Confessional absolutism thus produced results, but at a high price. In all those areas where the court's Catholic culture was not shared by the population at large, the new division fomented disaffection. In Upper Austria it provoked a dangerous peasant revolt in 1626; in Bohemia, which had already seen rebellions in defence of Protestantism in 1609, 1611 and 1618, another rising occurred in 1632. In the Ukraine (a part of the Polish Commonwealth after 1569), the Counter-Reformation imposed by the rulers in Krakow created a Catholic aristocracy using Latin script and speaking Polish, but left an Orthodox peasantry using Cyrillic and speaking the traditional language and this cultural separatism played an important role in providing popular support for the revolt of the Cossacks under Chmielnicki in 1648.

Even within the major creeds, reforms destabilized old loyalties and allowed escapist millenarian aspirations to develop. The Russian Orthodox Church underwent a thorough liturgical reform in the 1640s and 1650s as first Byzantine and then Ukrainian scholars introduced innovations; in 1653, this enforced 'westernization' provoked a serious split which lasted for several years. The Roman Catholic Church also moved towards schism over the Jansenist controversy concerning free will and good works while doctrinal disputes divided the Calvinists, from Scotland through Holland to Hungary.[47] But perhaps the most spectacular religious division of the age occurred in the widely dispersed Jewish community, which was torn in the 1660s by 'the most important messianic movement in Judaism since the destruction of the second Temple'.[48] Sabbatai Sevi, proclaimed Messiah and King of the Jews at Smyrna in 1665, found supporters from Scotland to the Yemen and from Morocco to Poland

before his apostasy the following year. Samuel Pepys, in February 1666, found even the hard-headed London Jews betting odds of 10 to 1 that ‘a certain person now at Smyrna be within two years owned by all the princes of the East…as king of the world’, while in Aberdeen Robert Boulter began to publish pamphlets about the imminent redemption of Israel. There were good reasons for this interest so far afield. Boulter was a millenarian, one of those who believed that the end of the world would come in 1666 (according to a prophecy in the Book of Daniel). He was convinced that, before this happened, the Messiah would come and redeem the Jews: Sabbatai Sevi therefore seemed to strengthen the millenarian case.

IV
AN INTERNATIONAL CONSPIRACY?

It is worth emphasizing this heightened religious awareness because, in early modern Europe, only religion could produce an ideology capable of uniting the opponents of a regime, of creating an alienated élite to champion those whose religion seemed threatened, and of mobilizing significant international support for the rebels. ‘Confessional absolutism’ could therefore lead to revolution because it threatened deeply entrenched and widely held beliefs. Thus the leaders of the anti-Habsburg factions in Austria, Bohemia and Moravia after 1609 almost all belonged to the beleaguered Calvinist minority, and strove to forge an alliance with their co-religionists elsewhere—a ‘Calvinist International’ based on Heidelberg, The Hague and Prague—to oppose what they perceived as a ‘Catholic International’ based on Rome, Vienna, Brussels and Madrid.[49]

Similar links connected other groups of rebellions. No-one can hope to understand the coming of the English Civil War without considering the National Covenant in Scotland and the revolt of 1641 in Ireland. The Stuart monarchy was a personal union of three crowns, and many policies of Thomas Wentworth, Earl of Strafford, were tried out in Ireland before they were attempted in England, while those of William Laud, Archbishop of Canterbury, were implemented simultaneously in England and Scotland. The opponents of Strafford in Dublin and London thus had as much in common as the English Puritans and Scottish Presbyterians who opposed Laud and, indeed, the pressure from Charles I’s ‘other’ kingdoms proved crucial in engineering the downfall of both ministers. Some Irish Catholics even claimed to have been inspired to rebel in October 1641 by ‘the example of Scotland’: if Scotland could defy Whitehall and make its own terms, they reasoned, then why not Ireland?[50] The Long Parliament’s opposition to the royal prerogative in 1640–1 depended heavily on the presence of the Scottish army in England; while in 1644–5, Parliament could hardly have defeated the king without Scottish military assistance. And the foundation upon which that Scottish assistance rested was the ‘Solemn League and Covenant’ which envisaged a Presbyterian (i.e. Scottish) religious settlement for England (and Ireland) ‘according to the word of God, and the examples of the

best reformed churches'.[51] By then, the Irish Confederates had also begun to look further afield, sending an ambassador to remind the King of Spain that:

> If we be trodden, noe man of judgment and righteousness will denye that heresye will not only prevaile but alsoe extinguish the orthodox faith [in] all the north partes of the world. The Huguenotts of France, Germany and Holland, and their correspondents, are not to be forgotten…[But with Spanish support] Ireland might from thenceforth be a great bulwarke against all the hereticks of the northerne partes of Europe.[52]

Religious fervour could not sustain rebellion indefinitely, however. A discussion between Queen Christina of Sweden and her councillors in February 1649, concerning a request for support from the exiled Charles II of Great Britain, neatly summarized the problem. Marshal Jakob de la Gardie argued first that in a period of such 'manifest *spiritus virtiginis*' all monarchs should co-operate, and suggested that co-religionists should support each other. But Queen Christina would have none of this: 'People use religion as a pretext', she asserted, 'and it is used by us against Calvinists and Catholics alike.' 'The Pope, the Spaniards and the rest of the House of Austria have always sought to make use of religion', de la Gardie reminded her. 'Like a raincoat when it's wet', quipped Christina. 'But one which may be put on in case of emergency', countered de la Gardie.[53] Religion certainly helped opponents of a government to overcome the moral, legal and philosophical objections to open rebellion, and could create at least the illusion of a common cause with coreligionists elsewhere. But, after a while, the exigencies of survival normally compelled the rebels to seek assistance wherever it could be found. Thus in the mid-seventeenth century the Dutch Republic, according to its official historian, always stood ready to acknowledge its *gemeyn interesse*, its common interest, with any other enemy of Spain, whether Protestant, Catholic, Orthodox, Moslem or Buddhist. And the staunchly Catholic leaders of the Neapolitan rebellion of 1647–8 expressly modelled their constitution on that of the Calvinist Dutch Republic—in December 1647 the Duke of Guise swore to defend the liberties of the 'Republic of Naples' just 'as the prince of Orange does in Holland', and supported moves to create a Neapolitan StatesGeneral.[54]

The same dilemma confronted most rulers. In 1640 Charles I, head of the Church of England, vainly tried to raise loans from various Catholic powers — from Spain (£300,000 in May, £100,000 in July, even £50,000 in August), France, Genoa and even the Papacy—in order to buy off the Scots without having to face his Protestant English subjects in Parliament. After the civil war began, aid came to him from the Catholic Queen Regent of France (Charles's sister-in-law) and the Calvinist prince of Orange (Charles's son-inlaw).[55] His son Charles II, in exile, also declared himself willing to accept assistance from any quarter, warning the doge of Venice that 'If all Christian princes do not make opportune provision…there is no doubt that the poisonous breath of rebellion will corrupt all peoples, far and near, wherever malcontents are found.' The same

point was made in the translation of a 'decree' issued by Tsar Alexis I of Russia that condemned the 'Rebellion of England as a universall Contagion, [which] being become epidemicall hath poisoned and infected most parts of Christendom', and called for a congress of European monarchs to be held in Antwerp in 1650 in order to form a league dedicated to the destruction of the Commonwealth. But the pamphlet was bogus—a royalist propaganda tract—just like the intriguing 'Interview between Sultan Ibrahim, Emperor of the Turks, and the King of England, held in the Elysian Fields', published in 1649 and discussing where the two rulers had gone wrong.[56]

Probably the Elysian Fields were the only place where Charles I and Ibrahim could have pooled their experiences. Communications in the seventeenth century moved too slowly to permit the effective exchange of resources, or even news. It took weeks for tidings of the uprisings in Constantinople, Muscovy and the Ukraine to reach England in 1648, and longer still for appropriate action to be discussed. The revolt of the Catholic settlers of Brazil against the Dutch, which broke out on 13 June 1645, did not become known in Amsterdam until late August, and a relief fleet left only in April 1646. Adequate reinforcements only set out from Holland on 26 December 1647, by which time Dutch Brazil had been lost.[57] Although news of violent upheavals in one area might make others elsewhere appear more serious, no 'revolutionary spirit' could ever connect the revolts in England or France, on the one hand, with those of Austria, Naples, Muscovy or the Ukraine on the other.[58]

V
A GLOBAL CRISIS?

But what of the Americas? After all, the pioneering study of Woodrow W. Borah, *New Spain's Century of Depression*, published in 1951, gave early expression to the idea of a seventeenth-century General Crisis in the entire Western hemisphere, and in 1960 the massive study of Pierre and Huguette Chaunu, based on the records of the customs authorities at Seville, pointed to a major depression in the trade between Spain and her American colonies between 1623 and 1650.[59] In 1974 Jonathan Israel presented evidence of an overall economic downturn in the viceroyalty of Mexico after 1620, and especially after the disastrous floods of 1629–34, when mining, textile manufacture, and trade with Europe, Peru and the Philippines all declined, just as fiscal pressure mounted. Political disturbances occurred in Mexico City in 1624, 1647 and 1664, each one seriously compromising the government's authority (especially in 1647 when the viceroy was forced to flee.)[60] Serious unrest also occurred in Peru between 1661 and 1667, centred on the silvermining community of Laicacota in the southeast, but also involving a conspiracy in Lima which, according to a contemporary, 'brought the kingdom within an ace of a great disaster'.[61] Peace soon returned, however, not least because the local élite proved increasingly adept at thwarting all government attempts to raise new taxes and export the proceeds to Spain.

Thus, although the central treasury in Lima sent over 100 million pesos to Castile between 1591 and 1690, 71 per cent left during the first four decades. Furthermore, although total revenues remained fairly stable between 1591 and 1650, the amount remitted to Seville declined from 64 to 42 per cent, while from 1651 to 1690 the total revenues declined and the amount sent to Spain fell dramatically to 5 per cent.[62]

How did these phenomena relate to developments in Europe? In 1969, André Gunder Frank, a powerful advocate of dependency theory, argued that contraction in Europe promoted economic self-sufficiency and diversification in the Americas; but in 1980, Immanuel Wallerstein argued that the economic downturn in the Old World during the seventeenth century led to stagnation in 'peripheral' zones like Spanish America.[63] Both these general studies remained uncluttered by primary research, however, and subsequent investigation suggests that the transatlantic trade declined mainly because Spain no longer produced the goods that the colonies needed, while Spain's remaining exports proved superfluous to colonial requirements, leading to the retention of more silver in the Americas. This major economic shift coincided with an expansion in the population of European descent and the increased participation by the surviving Amerindian population in regional markets. These four factors gradually produced a shift from primitive exportoriented production to a more diversified economic base. Both Mexico City and Lima founded their own Merchant Guild and established their own banks; agriculture became more efficient; local manufactures and intercolonial trade all burgeoned.[64] Data from French, English, Dutch and Portuguese possessions in the Americas concur: despite some minor reverses, the seventeenth century witnessed relatively smooth economic and demographic growth in all the European colonies along the Atlantic seaboard. The General Crisis in the New World proved relatively brief.[65]

In Chapter 10, Niels Steensgaard doubts whether East Asia's problems matched the scale of those that beset Europe. Certainly, in political terms, Japan survived the seventeenth century almost unscathed, with the authority of the Tokugawa government (the *bakufu*) becoming steadily more entrenched after the destruction of the Toyotomi clan in 1614–15. Serious rebellions, such as at Shimabara in 1637–8, occurred but seldom and proved relatively easy to suppress. In China, however, the successful rebellion of Li Tsu-Cheng against the Ming, followed by the prolonged and bloody campaigns of the Manchu to impose their authority on all parts of the 'central kingdom' lasted four decades (Taiwan only succumbed in 1683) and by the end, much of China had been devastated (see Chapter 9).[66] To the south, the aggression of the Dutch East India Company likewise caused widespread devastation, and several powerful maritime states in the Indonesian archipelago collapsed (see Chapter 8). In India, the savage succession war waged by Aurangzeb against his father and brothers during the 1650s likewise left a trail of destruction in the Mughal empire.[67] Taken together, *pace* Steensgaard, these various major political upheavals in

Asia and Europe during the midseventeenth century seem to raise questions of coincidence that might repay comparative study.

Jack A.Goldstone, a sociologist, has already attempted to do this. William S.Atwell, on pp. 241–5, discusses an early version of Goldstone's theories, published in 1988, but in 1991 a more complex and extensive examination appeared. *Revolution and Rebellion in the Early Modern World* considered four cases of early modern state breakdown in detail: England in 1640–2, France in 1788–9, and the collapse of the Ottoman and Ming empires in the mid-seventeenth century.[68] Starting from the premise that prolonged population growth in the context of relatively inflexible economic and social structures underlay all these phenomena, Goldstone discerned four key 'sub-themes'. First, severe pressure on state finances as inflation eroded revenues led to attempts to raise taxes which alienated important social groups yet failed to prevent increasing public debt. Second, popular unrest also increased as competition for land, urban migration, flooded labour markets, and declining real wages destabilized large segments of society. Third, intra-élite conflicts and factionalization grew, as inflation threatened the existing leaders while demographic growth produced ever more aspirants to élite positions. Fourth and finally, the emergence of ideologies of 'rectification and transformation' promised the reform, order and discipline which the Establishment had failed to provide.

Now the role of the four sub-themes in both the English and French Revolutions has long been recognized. Moreover, some of them also characterized the history of several major Asian states in the seventeenth century (see Chapters 8 to 10). The real issue is whether everything can be convincingly ascribed to rising population. The growth itself is not, of course, in doubt; the problem is the lack of unequivocal proof that demographic pressure in any direct sense polarized élites into factions (Goldstone's third sub-theme) or produced such phenomena as Puritanism or the Enlightenment (his fourth sub-theme). Nevertheless, as Goldstone noted, two periods of unusual demographic pressure occurred in early modern times: in the sixteenth and early seventeenth centuries, and in the late eighteenth and early nineteenth centuries. Both coincided with an age of revolution. 'In contrast', Goldstone observed, 'in the late seventeenth and early eighteenth centuries populations did not grow, and the basic process and its four sub-themes were absent. Political and social stability resulted.'[69] Finally, after 1850, industrialization and political reform in most Western states created a new flexibility which reduced the power of population pressure to trigger the crises that earlier had produced state breakdown.

Perhaps specialists in the different regions concerned will take up Goldstone's challenge: certainly the ball now rests in their court. It seems unlikely, however, that the same degree of congruity will be discovered for the causes of political revolutions as currently exists for explanations of economic dislocation (at least in the Old World). After all, as several contributors to this volume stress, no single model of revolution can explain even all the European upheavals of our

period. Both Ivo Schöffer and John Elliott rightly emphasize the exceptions to the 'crisis': Hamburg and north-west Germany largely escaped the Thirty Years' War and prospered, while even some areas regularly visited by troops managed to avoid serious damage by skilful adaptation to the soldiers' requirements;[70] the Dutch Republic flourished, and even the South Netherlands recovered from the more serious devastation of the later sixteenth century.[71] These exceptions prove nothing, however: even in 1848, the 'year of revolutions,' most areas of Europe did not experience political unrest! To be sure, in many ways 1848 did resemble 1648, as Guizot, fallen idol of the 'year of revolutions', was the first to point out: it came towards the end of a period of adverse climatic conditions, starting with the calamitous quinquennium 1812–17 (again apparently caused by a reduction in received solar energy, due to abnormally high volcanic activity ejecting dust into the stratosphere and blocking out the sun's heat); it followed a period of repeated lethal epidemics that affected all Eurasia (typhus and, above all, cholera); and it witnessed an era of popular disturbances and rebellions on an almost unprecedented scale.[72] But the link between these various events remains as elusive for the nineteenth, as for the seventeenth, century.

Lloyd Moote is surely correct to expect greater clarity to emerge only when more comparative studies have been completed.[73] Four obvious candidates for more detailed comparative treatment stand out: the revolts in Scotland, Ireland and England against Charles Stuart;[74] the various rebellions in France and Spain, comparing both the Fronde with the *huelga de los grandes* against Olivares in 1641–2, and the major provincial uprisings in the two countries;[75] the confrontations in Holland, Sweden and Denmark after the end of protracted periods of war;[76] and, finally, the unrest in Moscow, Constantinople and the Ukraine in and after 1648.[77] Until this further research has been done, we can scarcely improve on the verdict of Voltaire at the end of his 'global history', written more than 250 years ago: Three things exercise a constant influence over the minds of men: climate, government and religion'—even if we reject his determinist afterthought *'C'est la seule manière d'expliquer l'énigme de ce monde.'*[78]

NOTES

1 Many thanks to Christopher R.Friedrichs for commenting on an earlier draft of this introduction, and to Nancy E.Van Deusen for some important Latin American references.

2 Goodwin quoted by C.Hill, *Puritanism and Revolution* (London, 1959), p. 131; Whittaker by H.Trevor-Roper, 'The General Crisis of the seventeenth century', in T.S.Aston (ed.), *Crisis in Europe, 1560–1660* (London, 1965), p. 59; Milton in *Works of John Milton*, VIII (New York, 1933), pp. 14–15 (from *Second Defense of the People of England*, published in Latin in 1654). See also the opinion of the Venetian ambassador to Spain in 1648, who commented on the simultaneous crisis of the Spanish Monarchy on three continents, quoted in H.G.Koenigsberger, 'The

crisis of the seventeenth century: a farewell' in Koenigsberger, *Politicians and Virtuosi: Essays in Early Modern History* (London, 1986), pp. 149–68, at pp. 149–50; and of J.A.Salvius, quoted by G.Livet, *La Guerre de Trente Ans* (Paris, 1963), p. 121.

3 A.Macfarlane (ed.), *The Diary of Ralph Josselin, 1616–1683* (London, 1976), pp. 269–70. See similar entries for 1653 (pp. 294–5), 1654 (pp. 316–17), 1656 (pp. 359– 60), 1657 (pp. 389–90), and so on. See also pp. 648–57, 'Certaine remarkable things that fell out in my remembrance': the majority concerned foreign affairs. Josselin was right about the 'peace at Munster': the accords on demobilizing the Swedish army were not fully executed until 1654. See A.Oschmann, *Der Nürnberger Executionstag 1649–50. Das Ende des Dreißigjährigen Krieges in Deutschland* (Münster, 1991).

4 See J.H.Elliott, *The Count-Duke of Olivares: The Statesmen in an Age of Decline* (New Haven, CT, 1986), p. 659. The authors attributed all the misfortunes they described, including those affecting the Spanish Monarchy, to the 'universal hand of Providence'. Interestingly enough, their Chinese contemporaries came to the same conclusion regarding the misfortunes that overwhelmed the Ming dynasty: see W.S.Atwell, 'Ming observers of Ming decline: some Chinese views on the "seventeenth century crisis" in comparative perspective', *Journal of the Royal Asiatic Society of Great Britain and Ireland*, II (1988), pp. 316–48 at p. 325.

5 R.Mentet de Salmonet, *Histories des troubles de la Grande Bretagne* (Paris, 1649; English translation, London, 1753), p. ii. Salmonet wrote at the height of the crisis, when many anticipated that the fate of Strafford and Charles I in Britain would also befall Mazarin and Louis XIV.

6 R.T.Godfrey, *Wenceslaus Hollar: A Bohemian Artist in England* (New Haven, CT, 1994), 92; G.B.Birago Avogadro, *Delle historie memorabili che contiene le sollevatione di stato di nostri tempi* (Venice, 1653), reissued the following year in an expanded version, *Turbolenze di Europa dall' anno 1640 sino al 1650* (Venice, 1654: the accounts of the English and Portuguese troubles had expanded so much that they now became the subject of separate volumes); Lieuwe van Aitzema, *Saken van Staet ende Oorlog*, III (Amsterdam, 1669), pp. 230–1. Majolino Bisaccioni, *Historia delle guerre ciuili di questi ultimi tempi* (Venice, 1653), provided another 'guide to the crisis' for contemporaries.

7 T.Hobbes, *Behemoth*, ed. F.Tönnies (London, 1889), p. 1. Hobbes was refused permission to print his scurrilous account of the civil wars. Hobbes's contemporary, James Harrington, also saw the civil wars as part of a 'general crisis': see J.N. Shklar, 'Ideology-hunting: the case of James Harrington', *American Political Science Review*, LIII (1959), pp. 662–9.

8 Muley Ismail assassinated the last Sa'adian ruler of Morocco in 1660; Aurangzeb rebelled against his father and elder brother in 1658; and Li Tsu-Ch'eng deposed the last Ming emperor in 1644 only to fall to the Manchus. See p. 21 above also Chapter 9 below.

9 F.M.A. de Voltaire, *Essai sur les mœurs et l'esprit des nations* (Paris, 1756; 1963 edn.), II, pp. 756–7 and 794. On the originality of Voltaire's vision, see E.J.van Kley, 'Europe's "discovery" of China and the writing of world history', *American Historical Review*, LXXVI (1971), pp. 358–85.

10 One exception among modern writers is the Russian, B.F.Porshnev, *Frantzia, Angliskaya Revolutzia i Yevropeiskaya Politika v'Ceredina XVII* (Moscow, 1970), pp. 354–63 ('The crisis on a Eurasian scale').

11 T.K.Rabb, *The Struggle for Stability in Early Modern Europe* (Oxford, 1975), pp. 4–5. See also the subsequent syntheses and reviews of the literature in M.Hroch and J.Petrán, *Das 17 Jahrhundert—Krise der Feudalgesellschaft?* (Hamburg, 1981); P.J.Coveney, 'An Early Modern Crisis?' *Renaissance and Modern Studies*, XXVI (1982), pp. 1–26; L.M.Bilbao, 'La crisis del siglo XVII en su lectura económica. Un debate inconcluso', *Areas. Revista de ciencias sociales*, X (1989), pp. 51–72; D.E.Kaiser, *Politics and War: Sources and Consequences of European International Conflict, 1559–1945* (Cambridge, Mass., 1990), Chapter 1; and R.Romano, *Conjonctures opposées. La 'crise' du XVIIe siècle en Europe et en Amérique ibérique* (Geneva, 1992). See also the various essays concerning an earlier spate of unrest in Europe: P.Clark (ed.), *The European Crisis of the 1590s: Essays in Comparative History* (London, 1985); and a consideration of another simultaneous crisis in eastern and western Europe: J.V.Polišenský, *Tragic Triangle: The Netherlands, Spain and Bohemia, 1617–1621* (Prague, 1991).

12 The AAAS paper by Atwell appears here as Chapter 9; revised versions of the papers by Reid and Steensgaard appear as Chapters 8 and 10.

13 See J.A.Eddy, 'The case of the missing sunspots', *Scientific American* (May, 1977), pp. 80–92, and especially the figure on page 88 which calibrates the rise and fall of human civilizations with fluctuations in glacial activity, temperature changes and the sunspot and ^{14}C records in a fascinating way.

14 R.Mousnier, *Peasant Revolts in Seventeenth-Century France, Russia and China* (London, 1972; original French edn, 1967), which should be read in conjunction with the trenchant criticisms of A.L.Moote, M.O.Gately and J.E.Wills in *Past and Present*, LI (1971), pp. 63–80. P.T. Ho, *Studies on the Population of China, 1368–1953* (Cambridge, Mass., 1959), pp. 209, 236, 264–5, compared the consequences of the late Ming peasant wars in the 1630s and 1640s with the destruction caused by the Thirty Years' War in Germany, decimating the population and almost halving the area of cultivated land. See also the subsequent articles of S.A.M.Adshead, 'The seventeenth-century general crisis in China', *France-Asie*, XXIV (1970), pp. 251–65; H.Dunstan, 'The late Ming epidemics: a preliminary study', *Ch'ing-shih wen-t'i*, III.3 (Nov. 1975), pp. 1–59; F.E.Wakeman, 'China and the seventeenth-century crisis', *Late Imperial China*, VII.1 (June, 1986), pp. 1–26; and Chapter 9 in this volume.

15 John Selden's *Table Talk* (1639) quoted in K.V.Thomas, *Religion and the Decline of Magic* (London, 1971), p. 517.

16 For some details, see M.Baulant, E.Le Roy Ladurie and M.Demonet, 'Une syntèse provisoire: les vendanges du XVe au XIXe siècles', *Annales E.S.C.*, XXXIII (1978), pp. 763–71 (unusually late harvests in 1640–4 and 1648–51); and W.Lenke, *Klimadaten von 1621–50 nach Beobachtungen des Landgrafen Hermann IV. von Hessen* (Offenbach, 1960: Berichte des deutschen Wetterdienstes, LXIII): the Landgrave recorded an annual average of 74 days of frost in the 1640s (with a peak in 1649), compared with 58 days in the 1620s, and noted 157 days of precipitation in 1648 and 179 in 1649. For a useful overview see J.M.Grove, *The Little Ice Age* (London, 1988), and two collections of essays: K.Takaheshi and M.M. Yoshino (eds), *Climatic Change and Food Production* (Tokyo, 1978); and

R.I. Rotberg and T.K.Rabb (eds), *Climate and History: Studies in Interdisciplinary History* (Princeton, NJ, 1981). See also the quotation at p. 252, n. 50 below.

17 Opinions collected by G.B.Riccioli, *Algamestum novum, astronomian veteram novamque* (Bononiae, 1651), p. 96 (Book III, Chapter 3 is all about sunspots). For an interesting but mistaken attempt to tie the sunspot cycle into early modern human history, see J.N.Biraben, *Les hommes et la peste en France et dans les pays européens et méditerranéens,* I (Paris, 1975), pp. 120, 128 and 133–4, correlating plague visitations with a presumed sunspot cycle which, as Eddy shows, did not exist! The detailed drawings of mid-seventeenth-century astronomers—some of them daily compilations—also reveal that the sun rotated in a significantly different way from today: see J.A.Eddy, P.A.Gilman and D.E.Trotter, 'Solar rotation during the Maunder Minimum', *Solar Physics,* XLVI (1976), pp. 3–14.

18 Yi Tae-jin, 'Astronomical causes for the Little Ice Age (1500–1750): an analysis of the annals of the dynasty of Choson (Korea)' (A paper prepared for the International Congress of Historical Sciences, Montreal, 1995, graciously shared with us by Professor Yi.) For 1661 see pp. 25–6. For data on increased volcanic activity, see Grove, *Little Ice Age,* pp. 368–77, and pp. 242–3 below.

19 See also the evidence for India in S.Subrahmanyam, 'Persians, pilgrims and Portuguese. The travails of Masulipatnam shipping in the Western Indian Ocean, 1590–1665', *Modern Asian Studies,* XXII (1988), pp. 503–30, at pp. 513–14.

20 See J.C.Miller, 'The significance of drought, disease and famine in the agriculturally marginal zones of west-central Africa', *Journal of African History,* XXIII (1982), pp. 17–61.

21 F.Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II,* I (London, 1972), pp. 420–1. The quintal is 100 kg (220.5 lbs).

22 D.Pimental *et al.,* 'Energy and land constraints in food protein production', *Science,* CXC (1975), p. 760. In the United States today, one day's delay in harvesting reduces the yield of cereals by 63 kilos per hectare. For proof that a similar equation existed in early modern times, see A.-M.Piuz, 'Climat, récolte et vie des hommes à Genève, XVIe-XVIIIe siècles', *Annales E.S.C.,* XXIX (1974), pp. 599–618. Note, however, the dissenting voice of Pierre Vilar, who urged caution in using 'structural' explanations like climate to explain early modern crises of production: Vilar, 'Reflexions sur la "crise de l'ancien type", "l'inégalité des récoltes", et "sous-développement"', in *Conjoncture économique, structures sociales. Hommage a Ernest Labrousse* (Paris, 1974), pp. 37–58.

23 Grove, *Little Ice Age,* p. 13. See also pp. 407–12 on the desertion of upland farms in early modern Scotland and England, almost certainly as a result of climatic change. See also the data in P.R.Galloway, *Long-Term Fluctuations in Climate and Population in the Pre-Industrial Era* (Berkeley, Calif. 1986: *Population and development review,* XII no.1); and in M.Livi-Bacci, 'Chronologie, intensité et diffusion des crises de mortalité en Italie, 1600–1850', *Population,* XXXII (1977), pp. 401–40.

24 P.R.Galloway, 'Annual variations in deaths by age, deaths by cause, prices and weather in London, 1670 to 1830,' *Population Studies,* XXXIX (1985), pp. 487–505. See also the interesting study of E.Wöhlkens, *Pest und Ruhr im 16. und 17. Jahrhundert. Grundlagen einer statischen-topographischen Beschreibung der grossen Seuchen, inbesondere in der Stadt Ülzen* (Hannover, 1954), especially tables IX and XXIII. Of course some epidemic diseases, such as dyphtheria, typhus

and typhoid, spread independently of harvest yields: see H.Charbonneau and A. Larose (eds), *The Great Mortalities: Methodological Studies of Demographic Crises in the Past* (Liège, 1979), p. 339.

25 See the useful equation in J.de Vries, *The Economy of Europe in an Age of Crisis 1600–1750* (Cambridge, 1976), pp. 35–6, and the data on yield ratios in C.H. Wilson and G.Parker (eds), *Introduction to the Sources of European Economic History, 1500–1800* (London, 1977), pp. 10–11 and 121. For the 'high-level equilibrium trap' in China at this time, see M.Elvin, *The Pattern of the Chinese Past* (London, 1973), Chapter 13 (e.g., pp. 310–11).

26 M.Weisser, *The Peasants of the Montes: The Roots of Rural Rebellion in Spain* (Chicago, 1976), pp. 62–70.

27 Figures on Spanish towns from D.R.Ringrose, 'The impact of a new capital city: Madrid, Toledo and New Castile, 1560–1660', *Journal of Economic History*, XVIII (1973), pp. 761–91; on England, see P.Clark and P.A.Slack, *Crisis and Order in English Towns, 1500–1700* (London, 1972), p. 6. See also C.A.Corsini and G.Delille, 'La peste de 1656 dans le diocèse de Salerne. Quelques résultes et problèmes', in Charbonneau and Larose, *The Great Mortalities*, pp. 51–60, for an important discussion of the relative roles of migration and death in short-term population change.

28 On Cogenhoe, see H.Kamen, *The Iron Century, 1560–1660* (London, 1971), pp. 49–50; on Southampton, see T.B.James, 'The geographical origins and mobility of the inhabitants of Southampton, 1400–1600' (St Andrews University PhD thesis, 1977).

29 Data about beggars from Kamen, *Iron Century*, pp. 401–3. See also A.A.Parker, *Literature and the Delinquent: The Picaresque Novel in Spain and Europe, 1599–1753* (Edinburgh, 1967).

30 S.de Moncada, *Restauración política de España* (Madrid, 1619; ed. J.Vilar, Madrid, 1974), pp. 135–8. For subsequent corroboration, see V.Pérez Moreda, *Las crisis de mortalidad en la España interior. Siglos XVI–XIX* (Madrid, 1980); V. Pérez Moreda, 'The plague in Castile at the end of the sixteenth century and its consequences', in I.A.A.Thompson and B.Yun Casalilla (eds), *The Castilian Crisis of the Seventeenth Century: New Perspectives on the Economic and Social History of Seventeenth-Century Spain* (Cambridge, 1994), pp. 32–59; and C.Larquié, 'Popular uprisings in Spain in the mid-seventeenth century', *Renaissance and Modern Studies*, XXVI (1982), pp. 90–107.

31 F.Lebrun, *Les hommes et la mort en Anjou aux 17e 18e siècles. Essai de démographie et de psychologie historiques* (Paris-The Hague, 1971), p. 341.

32 Peter Laslett suggested that England was a special case. Laslett, *The World We Have Lost—Further Expanded* (London, 1983), Chapter 6, 'Did the peasants really starve?'. They certainly did in the North: see A.B.Appleby, 'Disease or famine? Mortality in Cumberland and Westmoreland, 1580–1640', *Economic History Review*, XXVI (1973), pp. 403–31; and also in Scotland: see M.Flinn (ed.) *Scottish Population History from the Seventeenth Century to the 1930s* (Cambridge, 1977), pp. 109–32. Nevertheless, some areas appear to have escaped hardship almost entirely, even in countries at the heart of the 'crisis': see, for one example, P. Saavedra, *Economía, política y sociedad en Galicia: la provincia de Mondoñedo, 1480–1830* (La Coruña, 1985), pp. 72ff.

33 G.Benecke made an early attempt to include Germany: 'The Thirty Years' War and its place in the General Crisis of the seventeenth century,' *Journal of European Economic History*, IX (1980), pp. 491–9; but his article was in reality a review of J. V.Polišenský, *War and Society in Europe, 1618–1648* (Cambridge, 1978).

34 R.Pillorget, *Les mouvements insurrectionnels de Provence entre 1596 et 1715* (Paris, 1975), p. 988.

35 C.R.Friedrichs, 'German town revolts and the seventeenth-century crisis,' *Renaissance and Modern Studies*, XXVI (1982), pp. 27–51; R.Dekker, *Holland in beroering. Oproeren in de 17e en 18e eeuw* (Baarn, 1982).

36 The Naples uprising of 1647 apparently constitutes an exception: see R.Villari, *La rivolta antispagnola a Napoli. Le origini (1585–1647)* (Bari, 1967), and S.Mastellone, 'Les révoltes de 1647 en Italie du sud. Étaient-ils paysannes ou urbaines?', *Revue d'histoire diplomatique*, XCII (1978), pp. 166–88, both noting the 'antifeudal' character of the revolt, at least in the countryside.

37 The role of ritual in the various uprisings in early modern Naples has been much discussed. Apart from Villari, *La Rivolta* and Mastellone, 'Les révoltes', see P. Burke, 'The Virgin of the Carmine and the revolt of Masaniello', *Past and Present*, XCIX (1983), pp. 3–21, and R.Villari, 'Masaniello: contemporary and recent interpretations', *Past and Present*, CVIII (1985), pp. 117–32.

38 A.Hoffman, 'Zur Geschichte der Schaunbergischen Reichslehen', *Mitteilungen des Oberösterreichischen Landesarchivs*, III (1954), pp. 381–436; K.McHardy, 'The rise of absolutism and noble rebellion in early modern Habsburg Austria, 1570 to 1620', *Comparative Studies in Society and History*, XXXIV (1992), pp. 407–38; Y.Bercé, *Histoire des croquants. Etude des soulèvements populaires au XVIIe siècle dans le sud-ouest de la France* (Geneva, 1974), pp. 648–51 (Bercé noted at least 450 popular revolts in Aquitaine between 1590 and 1715); and M.J.Stoyle, '"Pagans" or "paragons"? Images of the Cornish in the English Civil War', *English Historical Review*, CXI (1996), pp. 299–323, at p. 323. Certain areas may also have sustained a tradition of radical thought: C.Hill, *The World Turned Upside Down* (London, 1972), pp. 65–8 and 89–90, suggested that Kingston-upon-Thames and the villages around Pendle in Lancashire served as semi-permanent homes of English radicalism in the sixteenth and seventeenth centuries.

39 See K.M.Brown, 'Aristocratic finances and the origins of the Scottish Revolution,' *English Historical Review*, CIV (1989), pp. 46–87: quotation at p. 87, tax figures at p. 73. M.Lee. 'Scotland and the "General Crisis" of the seventeenth century,' *Scottish Historical Review*, LXIII (1984), pp. 136–54, took an opposite view; but Brown's evidence seems far more convincing.

40 See the graph of Bordeaux's increasing *taille* burden in R.J.Bonney, *Political Change in France under Richelieu and Mazarin, 1624–1661* (Oxford, 1978), p. 175. However, taxes levied did not necessarily mean taxes paid: some communities paid less than one-third of their assessment: see ibid. pp. 216–24, and J.B.Collins, *Fiscal Limits of Absolutism: Direct Taxation in Early Seventeenth-Century France* (Berkeley, Calif., 1988).

41 Figures from J.Black, *A Military Revolution? Military Change and European Society, 1550–1800* (London, 1991), p. 7; and J.A.Lynn, 'Recalculating French army growth', in C.J.Rogers (ed.), *The Military Revolution Debate* (Boulder, Col., 1995), p. 125. On the 'double helix' of growing army size and rising taxation, see

G.Parker, *The Military Revolution: Military Innovation and the Rise of the West, 1500–1800* (revised edn, Cambridge, 1996), p. 159.

42 The Dutch Republic and, to some extent, Sweden, proved the exception: both devised alternative means of spreading the burden of their increasing military costs —the former through an efficient system of long-term borrowing, the latter through raising taxes in conquered territories.

43 E.B.Sainsbury (ed.), *A Calendar of the Court Minutes etc of the East India Company, 1644–1649* (Oxford, 1912), p.i.

44 See the bar-graphs in G.Parker, *The Thirty Years' War* (2nd edn, London, 1996), p. 139, and F.Denton and W.Phillips, 'Some patterns in the history of violence', *Journal of Conflict Resolution,* XII (1968), pp. 182–95, at p. 191.

45 At least one contemporary writer also saw the early modern state as the principal 'revolutionary' force: see G.Naudé, *Considerations politiques sur les coups d'état* (Rome, 1623, ed. L.Marin, Paris, 1989). More recently, Rosario Villari has argued that popular revolts, too, were essentially conservative at this time: see Villari, *Ribelli e riformatori dal XVI al XVIII secolo* (Rome, 1979), Chapter 1.

46 Stephen Gosson quoted in P.W.Thomas, 'Two cultures? Court and country under Charles I', in C.Russell (ed.), *The Origins of the English Civil War* (London, 1973), p. 173. For more detail, see R.M.Smuts, *Court Culture and the Origins of a Royalist Tradition in Early Stuart England* (Philadelphia, 1987).

47 For a concise account of each, see G.Donaldson, The emergence of schism in seventeenth-century Scotland', in D.Baker (ed.), *Schism, Heresy and Religious Protest* (Cambridge, 1972: Studies in Church History, IV), pp. 277–94; J.Israel, *The Dutch Republic: Its Rise, Greatness and Fall* (Oxford, 1995), pp. 421–77; and L.Makkai, 'The Hungarian Puritans and the English Revolution', *Acta Historica* (Budapest), V (1958), pp. 13–44.

48 G.Scholem, *Sabbatai Sevi: the Mystical Messiah, 1626–76* (London, 1973), p.ix, on which the rest of this paragraph is based.

49 Parker, *The Thirty Years' War*, pp. 22–41. See, for another similar case, G.Parker, 'The Dutch Revolt and the polarization of international politics', in G.Parker, *Spain and the Netherlands, 1559–1659: Ten Studies* (revised edn, London, 1990), pp. 65–81.

50 See Nicholas Canny, 'What really happened in Ireland in 1641?', in J.H.Ohlmeyer (ed.), *Ireland from Independence to Occupation 1641–1660* (Cambridge, 1995), pp. 24–42, at p. 28.

51 See C.Russell, *The Scottish Party in English Parliaments 1640–1642, or the Myth of the English Revolution* (London, 1991); and The British problem and the English Civil War', *History,* LXXII (1987), pp. 395–415, reprinted in C.Russell, *Unrevolutionary England, 1603–1642* (London, 1992), Chapter 13.

52 J.T.Gilbert, *History of the Irish Confederation and the War in Ireland,* IV (Dublin, 1888), pp. 93–5: instructions of the Confederate Supreme Council to Hugh Bourke, 12 December 1644 OS. For the foreign support actually received by the Confederates, see the figures in Ohlmeyer, *Ireland from Independence to Occupation,* pp. 89–111.

53 S.Bergh (ed.), *Svenska riksrådets protokoll,* XIII (Stockholm, 1925), p. 17: debate for 21 February 1649 OS. See also the discussion of religion as a pretext for political action in Naudé, *Considerations politiques,* pp. 148–51 (interestingly, Naudé served briefly as an adviser to Queen Christina: ibid., p. 201).

54 Van Aitzema, *Saken van Staet*, II, p. 729, noted that the Dutch States-General established a committee to support the Catalans, in conjunction with France, as soon as news of their revolt arrived in 1640. Mastellone, 'Les révoltes', pp. 177–83, and Mastellone, 'Holland as a political model in Italy in the seventeenth century', *Bijdragen en mededelingen voor de geschiedenis der Nederlanden*, XCVIII (1983), pp. 568–82, records the 'example of Holland' during the 1647–8 revolt. For a Dutch dimension to the Naples revolt of 1585, see Villari, *La rivolta*, pp. 49–51; and R.Villari and G.Parker, *La política de Felipe II. Dos estudios* (Valladolid, 1996: Colección Sintesis, IX), pp. 38–9.

55 Information on Charles I's loans from S.R.Gardiner, *History of England*, IX (London, 1884), pp. 131–2, 157 and 184, and from P.Geyl, *Orange and Stuart* (London, 1969), Chapter 1. On foreign support for the Stuart cause, see the excellent study of I.Roy, 'Les puissances européennes et la chute de Charles I', *Revue d'histoire diplomatique*, XCII (1978), pp. 92–109; and, on the Stuarts and Hamburg, G.Schilfert, 'Zur Geschichte der Auswirkungen der englischen bürgerlichen Revolution auf Nordwestdeutschland', in F.Klein and J.Streisand (eds), *Beiträge zum neuen Geschichtsbild, zum 60. Geburtstag von Alfred Meusel* (Berlin, 1956), pp. 105–30.

56 See L.Loewenson, 'Did Russia intervene after the execution of Charles I?', *Bulletin of the Institute of Historical Research*, XVIII (1940), pp. 13–20; and Porshnev, *Frantzia*, p. 118.

57 W.J.van Hoboken, 'Een troepentransport naar Brazilië in 1647', *Tijdschrift voor Geschiedenis*, LXII (1949), pp. 100–9; and C.R.Boxer, *The Dutch in Brazil, 1624–1654* (Oxford, 1957), pp. 169–88. A proper account of the events of 1644 in China was not available in Europe until 1654, by which time Ming resistance had all but collapsed: E.J.van Kley, 'News from China: seventeenth-century European notices of the Manchu conquest', *Journal of Modern History*, XLV (1973), pp. 561–82; and E.J.van Kley, 'An alternative muse: the Manchu conquest of China in the literature of seventeenth-century northern Europe', *European Studies Review*, VI (1976), pp. 21–43. Note, however, that the Spanish authors of the *Nicandro* knew of the Manchu threat as early as 1643: see p. 2 above.

58 A point made by P.A.Knachel, *England and the Fronde* (Ithaca, New York, 1967), Chapter 1.

59 H. and P.Chaunu, *Séville et l'Atlantique, 1492–1650*, VIII, part 2 (Paris, 1960); see also A.García-Baquero González, 'Andalusia and the crisis of the Indies trade, 1610–1720', in Thompson and Yun Casalilla, *The Castilian Crisis*, Chapter 6.

60 J.I.Israel, 'Mexico and the General Crisis of the seventeenth century', *Past and Present*, LXIII (1974), pp. 33–5, reprinted in Israel, *Empires and Entrepôts: The Dutch, the Spanish Monarchy and the Jews, 1585–1713* (London, 1990), Chapter 11. See also Chapter 10 in the same collection: 'Olivares and the government of the Spanish Indies'. The controversial article of J.J.TePaske and H.S.Klein, 'The seventeenth-century crisis in New Spain: myth or reality?', *Past and Present*, XC (1981), pp. 116–35, which sought to measure trends in overall economic activity through fluctuations in treasury receipts, has been severely criticized: see H.Kamen and J.Israel, with a rejoinder by TePaske and Klein, 'Debate: the seventeenth-century crisis in New Spain: myth or reality?' *Past and Present*, XCVII (1982), pp. 144–61. However, the treasury figures themselves have great value: see TePaske

and Klein, *The Royal Treasuries of the Spanish Empire in America* (Durham, North Carolina, 1982).

61 L.M.Glave, *Trajinantes. Caminos indigenas en la sociedad colonial, siglos XVI/XVII* (Lima, 1989), pp. 198–205, quotation on p. 199, n. 31. Further north, the colonies of the isthmus experienced marked economic recession without political repercussions. See M.J.McLeod, *Spanish Central America: A Socio-Economic History, 1520–1720* (Berkeley, Calif. 1973). pp. 264–329.

62 K.J.Andrien, *Crisis and Decline: the Viceroyalty of Peru in the Seventeenth Century* (Albuquerque, 1985), especially the table on p. 34.

63 A.G.Frank, *Latin America, Underdevelopment or Revolution* (London, 1969), pp. 3–30; I.Wallerstein, *The Modern World System. II. Mercantilism and the Consolidation of the European World Economy, 1600–1725* (New York, 1980), pp. 144–55.

64 For developments in Peru, for example, see Andrien, *Crisis and Decline*, pp. 1–41; K.J.Andrien, *The Kingdom of Quito, 1690–1830: The State and Regional Development* (Cambridge, 1995), pp. 15–32; and M.Suárez, 'Monopolio, comercio directo y fraude: la elite mercantil de Lima en la primera mitad del siglo XVII,' *Revista andina*, XI (1993–4), pp. 487–98.

65 See Romano, *Conjonctures opposées*, for numerous statistics. Although sketchy evidence of an ecological disaster exists for the Great Plains of North America in the mid-seventeenth century—migration of some tribes, settlements abandoned we have unfortunately located no systematic review of the data. See, however, the evidence from the Southwestern United States at p. 297, n. 102 below.

66 See F.E.Wakeman, *The Great Enterprise: The Manchu Reconstruction of Imperial Order in Seventeenth-Century China* (2 vols, Berkeley, Calif., 1985), and L.A. Struve, *The Southern Ming, 1644–62* (New Haven, CT, 1984).

67 Strangely, the war remains unmentioned by J.F.Richards, The seventeenthcentury crisis in South Asia', *Modern Asian Studies*, XXIV (1990), pp. 625–38. For details, see J.F.Richards, *The New Cambridge History of India. I.5 The Mughal Empire* (Cambridge, 1993), pp. 151–64. The same decade also saw a bitter struggle between the Dutch and the Portuguese in Sri Lanka and southern India, which again caused widespread destruction before the Dutch emerged victorious.

68 Jack A.Goldstone, *Revolution and Rebellion in the Early Modern World* (Berkeley, Calif., 1991). See also the synopsis of the argument in Goldstone, 'The causes of long waves in early modern economic history', *Research in Economic History. Supplement*. VI (1991), pp. 51–92.

69 Goldstone, *Revolution and Rebellion*, p. 460.

70 See C.R.Friedrichs, 'The war and German society', in Parker, *The Thirty Years' War*, pp. 186–92, and references on pp. 271–2.

71 Reports of the 'political crisis' in both regions have been exaggerated: see H.H. Rowen, The revolution that wasn't: the coup d'état of 1650 in Holland', *European Studies Review*, IV (1974), pp. 99–117; and P.Janssens, 'L'échec des tentatives de soulèvement dans les Pays-Bas méridionaux sous Philippe IV (1621–1665)', *Revue d'histoire diplomatique*, XCII (1978), pp. 110–29.

72 See J.D.Post, 'Meteorological history', *Journal of Interdisciplinary History*, III (1973), pp. 730–1. Guizot's analogy was expertly demolished by Karl Marx, *Selected Essays*, ed. H.J.Stenning (Edinburgh, 1926), pp. 196–208.

73 See the pioneering comparative articles of C.S.L.Davies, 'Peasant revolts in France and England: a comparison', *The Agricultural History Review*, XXI (1973), pp. 122–34; J.H.Elliott, 'England and Europe: a common malady?', in Russell, *The Origins of the English Civil War*, pp. 246–57; R. Pillorget, 'La crise européenne du XVIIe siècle (1640–1660), *Revue d'histoire diplomatique*, XCII (1978), pp. 5–16; and C.Russell, 'Monarchies, wars and estates in England, France and Spain, c. 1580-c. 1640', *Legislative Studies Quarterly*, VII (1982), pp. 205–20 (reprinted in Russell, *Unrevolutionary England*, Chapter 7). See also Mousnier's study, *Peasant Revolts*, discussed in note 14 above.

74 See C.Russell, *The Causes of the English Civil War* (Oxford, 1990), and *The Fall of the British Monarchies, 1637–1642* (Oxford, 1991). Although fascinating, neither work incorporates much recent research on Irish developments.

75 See S.J.Woolf, 'La crisi della monarchia spagnola: le rivoluzioni degli anni 1640–1650', *Studi Storichi*, IV (1963), pp. 433–48; Thompson and Yun Casalilla, *The Castilian Crisis*, especially Chapter 14; and *1640. La monarquia hispánica en crisis* (Barcelona, 1992).

76 See, respectively, Rowen, 'The revolution that wasn't'; M.Roberts, 'Queen Christina and the General Crisis', in Aston, *Crisis in Europe*, Chapter 8, and C. Nordmann, 'La crise de la Suède au temps de Christine et de Charles X Gustave (1644–1660)', *Revue d'histoire diplomatique*, XCII (1978), pp. 210–32.

77 See, respectively, L.Loewensen, 'The Moscow uprising of 1648', *Slavic and East European Review*, XXVII (1948–9), pp. 146–56; B.McGowan, *Economic Life in Ottoman Europe: Taxation, Trade and the Struggle for Land, 1600–1800* (Cambridge, 1981); and F.E.Sysyn, *Between Poland and the Ukraine: the Dilemma of Adam Kysil, 1600–1653* (Cambridge, Mass, 1986).

78 Voltaire, *Essai*, II, p. 806.

2
THE SEVENTEENTH-CENTURY CRISIS*

Niels Steensgaard

In his introduction to the anthology, *Crisis in Europe 1560–1660,* published in 1965, Christopher Hill maintained that agreement now seemed to have been reached that there was an economic and political crisis all over western and central Europe in the seventeenth century. This is undoubtedly correct: the crisis has been an undisputed fact among those historians who are occupied with early modern Europe; it has become the hallmark of the seventeenth century in the same way as the Renaissance and the Reformation characterize the sixteenth century and Enlightenment and Revolution the eighteenth century. But agreement does not lie very deep; historians are agreed about the existence of the crisis, but not about its character. Since it first became recognized in the middle of the 1950s, the term 'seventeenth-century crisis' has been employed in at least four different senses.

1 A general economic crisis, i.e., a retrogression in European production, or at any rate a fall in the rate of growth of the European economy. This aspect has especially concerned recent French historians engaged in the economic field of research,[1] but forms a part of all versions of the crisis theory.
2 A general political crisis, i.e., a crisis in the relationship between state and society. This theory, conceived by Trevor-Roper, takes as its point of departure the contemporaneous revolutions in the middle of the century, the economic crisis being regarded as an established fact. According to Trevor-Roper the crisis was the result of a conflict between a puritanically minded opposition (the 'country') and a parasitic bureaucracy created by the Renaissance state during the boom of the sixteenth century, but which became unendurable during the period of decline and the lengthy wars in the seventeenth century.[2]
3 A crisis in the development of capitalism. For Eric Hobsbawm the crisis was a symptom of the decisive break between the feudal order of society and the capitalist production forms.[3] This theory is developed in the framework of Marxist terminology, though it cannot be regarded as *the* Marxist interpretation of the crisis.[4]

4 A crisis comprising all aspects of human life. For Roland Mousnier, the crisis is not so much a problem demanding a special explanation, as a useful term with which to describe a chaotic century.[5]

Finally, a fifth group might be said to comprise those historians who express doubts as to the justifiability of such a concept as the seventeenthcentury crisis. A.D.Lublinskaya has been the one to reject the various versions of the crisis theory the most emphatically and in the most detail, but the Dutch historian, Ivo Schöffer, has also expressed his misgivings in describing the 'golden century' of The Netherlands as the century of the European crisis.[6]

Since the versions of the crisis theory summarized above were elaborated, our knowledge of the seventeenth century has grown considerably; inspired to some extent by the crisis theory, a number of historians have worked on the problem within the fields of both economic and political history. Despite the fact that they have often taken one or other of the original formulations of the crisis theory as their starting-point, the concept cannot be said to have been clarified. It must be realized that nowadays the crisis is often merely an affirmation of the undisputable fact that something happened in the seventeenth century; the crisis has become a synonym for what historians concerned with other centuries call 'history'. It has therefore become necessary to reconsider the very concept of crisis; to seek—in the light of recent research— clarification as to whether the economic and political conditions of the seventeenth century justify speaking of a general crisis; and, if this is the case, to attempt to define the nature of this crisis more narrowly.

I
THE ECONOMIC CRISIS

'Et le XVIIe siècle n'est un siècle "triste" qu'à condition de définir scientifiquement une tristesse de longue durée, s'il en existe.' ('And the seventeenth century is only "sad" provided that we can define in scientific terms what we mean by "long-term sadness", if such a thing exists.')[7] But how does one define a 'tristesse de longue durée'? The answer, presupposing unlimited access to statistical material, is simple: the definition must be bound up with the series concerned with consumption, production, employment, economic growth, etc.; and the detection of both long- and short-term fluctuations must be based on these series.

Such a procedure is excluded in the study of the seventeenth century. The idea of a prolonged economic depression originates from the statistical era, but the method must be adapted to the conditions of the pre-statistical era; we cannot procure the statistical series we need, so we must use what we have. It must nevertheless be an elementary requirement as to method that the indicative facts from which we arrive at the European fluctuations must be drawn from the whole of Europe, or that we at least have a fairly certain knowledge as to the

extent of the gaps. Conclusions about the general fluctuations of the European economy drawn from isolated or specialized series are *a priori* inadmissible. This is something that historians have often forgotten,[8] but it should be added that only the most recent research has made it possible to comply. In what follows, I shall examine the present state of research in five sectors—population, agriculture, industry, international trade and the public sector—taking heed of the necessity of covering (or almost covering) the total European development.

Population

Of especial interest for the detection of a prolonged crisis are the changes in the total population figures. There is scarcely any doubt now that the increase in population during the sixteenth century was followed in the seventeenth century by a decline, by stagnation, or at any rate by a retardation in the rate of growth. The demographic peak seems to have been reached earlier in southern Europe than in the north, so that throughout the century a shift in the balance of the population occurred from the Mediterranean towards the Channel regions. Castile, the Italian peninsula (though not the islands) and Germany suffered a considerable decline in population in the first half of the seventeenth century.[9] The population of Catalonia continued to increase slightly in the first part of the seventeenth century, but from about 1630 onwards it stagnated.[10] Both the south and the north of The Netherlands seem to have suffered a corresponding fate, the turning-point nevertheless lying nearer the middle of the century.[11] Denmark and Poland suffered a considerable loss of population in connection with the Northern War at the end of the 1650s.[12] England's population is supposed to have increased in the seventeenth century, but it is probable that the increase took place chiefly in the first half of the century.[13] As far as France is concerned, the century began with an increase in population, which must nevertheless be seen in the light of the losses incurred during the Wars of Religion at the close of the sixteenth century. Further development shows quite considerable regional variations, but the general impression is nevertheless that of a moderate increase in population until the middle of the century in northern France, continuing until 1675–80 in southern France, and thereafter stagnation or a decline.[14] In 1693, after the 'hunger year', France is said to have had the same population as she had a century earlier at the close of the Wars of Religion.[15]

It has previously been the general opinion that the death rate was the most important variable in the pre-industrial population figures, and it has therefore been possible to bring the population trends in direct relation to the general economic development only in connection with the great catastrophes: famine, plague and war, i.e., within the short run only. Therefore, the fact that it has been possible to demonstrate far greater fluctuations in the birth rate than previously supposed awakens considerable interest. There is evidence of such a considerable span, both in time and from place to place, that historians have dared without hesitation to speak of deliberate family planning.[16] The most

important variable in this respect is women's age on first entering marriage, but the number of pregnancies within marriage also shows fluctuations, which must be the outcome of decisions founded upon either economic considerations[17] or *'mentalités collectives'*.[18] This observation paves the way for a more human and less mechanical form of demography. Wrigley has suggested that the Malthusian model, under which populations tend to approach a maximum with regard to accessible foodstuffs, is an exception, fluctuations *below* maximum being the rule;[19] and he has expressed the opinion that birth control created the basis for an increase in real incomes in England between the middle of the seventeenth century and the middle of the nineteenth century.[20]

Agriculture

In order to undertake a comprehensive evaluation of agricultural trends during the seventeenth century (as opposed to detailed local studies), there are at present only two possible methods, based either upon price history or upon the study of the yield ratios. Supplementary information may be gained from the data concerning the corn trade.

Already several years ago, on the basis of price historical information derived from all over Europe, Abel painted a picture of the long-term trends in European agriculture that he has by and large been able to maintain in the revised edition of the book published in 1966.[21] He pointed out that the turning-point of the sixteenth-century boom had already been reached in the years immediately following 1600. The outbreak of the Thirty Years' War again brought rising prices, which continued in Denmark, France and northern Italy until the 1620s, in Germany and Holland until the 1630s and in England, Belgium and Austria until the 1640s. Prices thereafter were low all over Europe until the middle of the eighteenth century, although in Germany a tendency to rise can be demonstrated somewhat earlier. Abel seeks the explanation of this prolonged depression first and foremost in demographic developments.

Abel's material is not always as reliable as his methods (for instance, as far as France is concerned he relies on d'Avenel's out-dated price figures), but in the main his results have been confirmed by recent studies of the history of prices.[22] There is, however, no agreement concerning the time when agricultural prices began to fall; French historians have ardently discussed the exact date;[23] while Abel himself has advocated that, instead of seeking a definite turning-point, the reversal should be regarded as having taken place over a longer period.[24]

In the yield ratios calculated by the Dutch historian Slicher van Bath, we have obtained a valuable supplement to the price-historical investigations.[25] Van Bath operates with fifty-year periods—so there is no question of a search for turning-points in this case—and he argues for a decline in the yield ratio in Germany in the second half of the sixteenth century; in England, Germany, France and eastern Europe in the first half of the seventeenth century; and in England, France, Germany and Scandinavia in the second half of the seventeenth

century.[26] Several historians have expressed misgivings concerning van Bath's extremely heterogeneous material,[27] and it is indisputable that more data and a more penetrating criticism of the material gathered would increase confidence in his conclusions. On the other hand, the fluctuations established appear so significant that even on the present basis it is safe to assume that the demonstrated tendencies are in agreement with the actual development. The fact that the decline in the yield ratio coincided with stagnating or falling prices indicates that the cause must be sought on the side of demand.[28]

Abel's and van Bath's results are confirmed by the information available concerning the extent of the international corn trade. Aksel E.Christensen demonstrated considerable short-term fluctuations but no sign of a lasting retrogression in the transport of corn through the Sound before the end of the 1630s.[29] On the other hand, Faber and Jeannin have proved that, after the middle of the seventeenth century, there was a considerable reduction in the export of corn from the Baltic region.[30] Faber, too, chooses to seek the operative factor on the side of demand, i.e., in the demographic conditions, but he also points out that the cultivation in western Europe of new crops like maize, rice and buckwheat may have been increasing. This latter hypothesis is supported by the French historian Jaquand.[31] It would also be reasonable in this connection to consider the fact that England, after the introduction of the bounty in 1673, began to assert herself as an exporter of corn to the west European market.[32]

Industry

If we are to fulfil the requirement that the data utilized should be applicable to Europe as a whole, then there is in this case only one suitable industry. However, it is one of the most important: the weaving of woollen textiles. The available figures have to a large extent already been taken into account in the discussion concerning the chronological limits and the very existence of the crisis. A survey of the available material, however, suggests that the conclusions reached on the basis of isolated series have been hasty: although the material shows great fluctuations, I do not believe that it supports the theory of a general European crisis.

The decline of the traditional centres of the Italian wool industry starts around 1600 and is indisputable some decades into the new century.[33] The Castilian wool industry stagnated from about the end of the sixteenth century, but there seems to have been no question of a real depression until the middle of the seventeenth century.[34] The Catalan industry seems to have been in difficulties from about 1620.[35]

Corresponding to this recession in the Mediterranean region, there was an increase in north-west European production and in the export of woollen textiles to southern Europe, the Levant and Asia.[36] The production of the most important Dutch textile centre, Leiden, was growing until 1654, the value of annual production rising from 1630 until that date from about 4 million fl. to 9 million

fl. Lying concealed behind this overall development in Leiden's production, there is however a qualitative reorganization. The Netherlands was concentrating to an increasing extent on the production of more expensive goods; the production of the cheaper new draperies began to decline as early as the 1620s.[37] However, total Dutch textile exports to the Baltic continued to increase up to the 1640s.

There are no proper production data as far as England is concerned, the most valuable statistics being those concerning the export of cloth from London.[38] Although the series is not complete (seventeen years within the period 1601–40), it is clearly characterized by stagnation after the peak year of 1614. It ought, however, to be added that the value of the consignments undoubtedly was increasing, since a steadily larger proportion of the cloth was exported finished and dyed and not—as previously—as half-finished white cloths.[39]

But these export figures that show stagnation comprise cloths alone; they do not include the lighter and cheaper 'new draperies', which were produced and exported in increasing quantities during the same period and which by 1640 are thought to have constituted almost as large a percentage of English exports as the traditional broadcloths.[40] On the basis of Hinton's survey of all the cloth exported through the Sound, Romano asserts (see Chapter 7, p. 172 below) that the peak period of English exports, not just of cloth but of woollen textiles as a whole, came in the second decade of the century.[41] This may be an overstatement, but there was certainly a prolonged rise in Dutch textile exports to the Baltic (which continued up to the 1640s). The changeover from production of broadcloths to production of new draperies constituted a decisive structural crisis in the development of the English textile industry, but there is absolutely no justification for interpreting this as a symptom of a European regression: the opposite trends in English and Dutch industry point to the contrary. Wilson finds the causes of the complementary development chiefly in the English manufacturers' easier access to cheap long-staple wool as well as in the higher production costs (especially wages) in the Netherlands. In cheaper goods, The Netherlands were unable to compete with the English rural industry.[42]

In the southern Netherlands the century also began with a rapid advance for the wool industry. Lille prospered until about 1620, but suffered a recession in the 1630s.[43] The most important production centre, Hondschoote, trebled its exports between the 1590s and the 1630s, the peak point of the seventeenth century being reached in 1630; not until the years 1640–5 was there any question of a catastrophic recession.[44] In Ghent and Bruges the production of cheaper-quality textiles ceased, because the town weavers could not compete as regards price with the rural industry; but on the other hand, they demonstrated a considerable adaptability by switching over to the production of luxury textiles. According to Craeybeckx, the textile industry continued to prosper in both these towns until the last decades of the seventeenth century when the increased protectionism on the export market was largely responsible for reducing sales.[45]

It is not possible to procure any reliable production or export statistics for France, but with the aid of other quantitative sources French historians have been able to assess the development of the wool industry of northern France in the seventeenth century. Until 1610 there was a rapid advance, albeit in part no more than a recovery after the slump that accompanied the last stage of the Wars of Religion. Progress continued at a slower rate until the 1620s but in about 1630 the industry suffered a severe crisis which continued until the mid-1640s. There was some recovery in 1648 and 1659, and from 1660 onwards there was a slow and hesitant progress characterized by a changeover to luxury production. In France, too, the manufacture of cheaper goods was transferred to country districts.[46] A corresponding development can be detected in the relationship between Aachen and its surrounding countryside.[47]

There are many missing details in this picture of the European wool industry, but one thing is certain: it is not possible to reach any conclusions regarding the general European trends on the basis of isolated local or national statistical series. It is not difficult to demonstrate local crises, or crises in the production of specific qualities, but where is one to place the general European crisis of the seventeenth century? In the first years of the century—if we are to go on the Italian or Castilian data; in 1614—if we are to go on the export of English cloth; at the beginning of the 1630s—if we are to go on the northern French industry; in 1640–5—if we accept Hondschoote as a norm; in 1654—if we are to go on Leiden. But at that time the reconstruction of the French textile industry was drawing near, and Ghent's and Bruges's difficulties did not come about until after the revival of the French textile industry. Finally, the English export of new draperies seems to have been steadily increasing, and it is probable that throughout the century there was an increase in the production of woollen materials in northern and eastern Europe.[48]

Two structural alterations in the textile industry appear just as significant as the demonstrable fluctuations in production: the conversion to rural industry, and the change-over to the production of lighter textile materials. Everywhere, the seventeenth century was an important phase in the development of rural industry on a 'putting-out' basis,[49] and it is thus probable that the stagnation or retrogression of industry in the towns often concealed a transference of production to the country districts. But since one of the chief motives for such a transference was precisely the wish to dodge the guild regulations and taxation of the towns—i.e., the institutions that have provided us with the bulk of the quantitative sources—we shall scarcely ever with any certainty be able to determine the extent and chronology of this transference. Nor can we determine precisely the other structural change—the altered taste with regard to cloth—but the general tendency is clearly from the heavier cloth towards the lighter woollen or mixed cloths, worsteds and silk, and—in the second half of the century—the Indian cottons.

International trade

Nor do the statistics available for international trade allow unequivocal conclusions regarding the overall trends. Let us begin with the trade between Seville and Spanish America. The decline demonstrated by Chaunu is indisputable: the peak was reached in 1608, the thirteen-year moving average culminated in 1614–15, and thereafter there was a drop until the middle of the century in the annual tonnage dispatched to a level somewhat below half of the maximum.[50] But Chaunu stopped at 1650 and Hamilton collected figures on the import of American silver to Spain only up to 1660; and both assumed, with little evidence, that the depression continued throughout the second half of the seventeenth century.[51] However, Morineau has subsequently demonstrated that the import of silver from Spanish America to Seville between 1661 and 1700 was far greater than previously reckoned; it even equalled the record figures from the end of the sixteenth century.52 Moreover, we must remember that these figures are limited to *Spanish* America. As Chaunu himself has pointed out, from the beginning of the seventeenth century onwards there were 'other Americas'. The production and export of sugar from Brazil were increasing rapidly, and the English and the Dutch began to interest themselves to a greater extent in America precisely in those years when the Spanish trade was in decline. The new competitors could scarcely make up for the decline in the trade of Seville in the first decades of the century, but it is probable that they did so shortly after the middle of the century.[53]

The Sound Toll Registers, as is well known, are difficult to interpret; exceptions and smuggling limit the value of the information given by the tables, and only after correction for these and other sources of error are they of interest as a barometer registering fluctuations. Aksel E.Christensen found no signs of permanent recession in the period he investigated, i.e., until 1639.[54] This impression is confirmed by Jeannin, who has carried the critical analysis of the Sound Toll Registers further by means of a comparison with the toll registers from Königsberg.[55] In his corrected series there are signs of a trade depression in the Baltic only from about the middle of the century. The years between 1620 and 1650 may have been characterized by violent fluctuations, but according to Jeannin there is no question of a lasting depression, as the setbacks can be traced to short-term production crises, i.e., crop failure in the Baltic regions, or to political conditions. On the other hand, the 1650s and 1660s exhibit signs of real depression; not until the years between 1668 and 1680 do the various commodities, individually, show signs of recovery. Not all groups of commodities were equally badly hit by the recession. Hardest hit was corn, whereas raw materials for industry and ship-building materials did not manifest nearly the same retrogression, either in price or in turnover.[56] Jeannin concludes that the adverse trend did not hinder a considerable advance in important industrial sectors.[57]

The trade to Asia has also been drawn into the discussion concerning the economic crisis.[58] The same applies here as in other places, however: all argument based on isolated series is misleading. If the total number of ships sent out from Europe in the sixteenth and seventeenth centuries with an Asian harbour as destination is counted up, a trend that runs more or less counter to the other known series for the total European trade is revealed. Stagnation or a slight increase from the middle of the sixteenth century is succeeded by a rapid expansion from 1600 to 1620, i.e., coinciding with the Seville trade recession. A period of stagnation, 1621–50, is then succeeded by a new largescale expansion, 1651–70, i.e., coinciding with the greatest difficulties in the Baltic trade. An interesting feature disclosed by the survey is that, just as in the case of the wool industry, a clear complementary relationship between the English and the Dutch activity can be detected, at any rate in the last four decades of the seventeenth century.[59]

This complementary relationship, so obvious at the time, has perhaps been underestimated by the historians of our age who are more used to analysing economic life in terms of growth or fluctuations.[60] The decades in the middle of the century, when the greatest economic difficulties were to be found in Spain, Germany, France and England, were at the same time the golden age of the Netherlands.[61] When the Dutch trade began in the last years of the century to show signs of weakness—the decline may be dated from 1672 at the earliest[62]—there were others prepared to step in: the advance of English foreign trade after 1660 is a well-known phenomenon. According to R.Davis, during the last four decades of the century English exports rose by about 50 per cent, imports by about 30 per cent;[63] and according to Delumeau, French foreign trade and shipping were also expanding from 1660 to 1690 to an extent which makes it unreasonable to connect France as a whole with *'la conjoncture méridionale'*.[64] Finally, Danish-Norwegian shipping and trade were also advancing in the last decades of the century.[65]

The public sector

In the relevant literature concerning the seventeenth century's economic crisis, a description of the public sector may be sought in vain. This omission appears unjustified: a modern analysis would not be able to avoid including public consumption and public investments, and when an economy like that of seventeenth-century Europe with its poorly developed market sector is concerned, the omission is fatal. Not only were the governments in seventeenth-century Europe undoubtedly the strongest buyers in the market, but there is also every probability that they had proportionately a greater share in the total market transactions than has been the case in the industrialized economies until quite recently. Moreover, the redistribution that was effected by the State was of such an extent that it would necessarily affect the standard of living within the

separate regions and groups: it is therefore of additional interest in interpreting the production and price data we have at our disposal.[66]

That historians have nevertheless tended to avoid this complicated problem may possibly be due to the nature of the material. The budgets and surveys of revenue and expenditure, which either have been or can be unearthed from the various European states in the seventeenth century, may be correct enough within their own limitations, but they are seldom comparable over a long period of time and they give only an incomplete picture of the economic impact of the State. To the money at the disposal of governments should be added the legitimate remunerations and perquisites of governmental officials, tax collectors and tax farmers, monopoly profits, payments for delegated state services, corruption, looting of conquered towns and countryside, etc.

Many historians have indicated in general terms the possibility that wars and taxes were contributory causes of the economic difficulties and social conflicts of the seventeenth century,[67] but only a few have tried to estimate the extent of the taxes in relation to the total production. Fernand Braudel tentatively estimated the gross product of the Mediterranean region in the years approaching 1600, with a population of about 60 million, at about 1200– 1500 million ducats. The total state budgets during the same period he estimated at about 48 million ducats, i.e., less than a ducat per head per annum— about the same as the average contribution to the seigneur. Braudel is of the opinion that the part of the 'product' that was administered through the public sector was astonishingly small: 'Was the mighty state, striding across the stage of history, no more than this?' he asks.[68] Of course it was. As Braudel himself points out, the greatest part of the estimated product never reached the market: it was consumed locally. Annual grain consumption alone was equivalent to between one-third and one-half of the total estimated product. Moreover, we must remember that up to 90 per cent of the population lived in rural areas. Early modern Europe was, to a large extent, a 'subsistence economy', and the role of the State as an entrepreneur was correspondingly greater.

Ortiz Domínguez has attempted a corresponding calculation for Castile alone, in the middle of the seventeenth century. He estimates the gross national product at about 180 million ducats, and the total amount demanded in taxes at about 20 million ducats or approximately 11 per cent of the latter. Nor is Ortiz Domínguez impressed by the size of the share—the twentieth century has toughened us—but, on the other hand, he is in no doubt that taxation was disastrous for the Castilian economy. He finds the most important reason for this is the unequal distribution of taxes and in the fact that, over and above the Crown's demands, the Castilian population was saddled with a number of other expenses in what we would call today the public sector. The foremost of these extra burdens was the tithe.[69]

Braudel has also tried to collect information concerning the development of the states' budgets during the sixteenth century. He emphasizes the shortcomings of the available information but nevertheless discerns a connection between the

economic trend and the state budgets. 'Despite their lacunae, these curves show that fluctuations in state revenues corresponded to fluctuations in the price sector,' he wrote.[70] The two curves certainly move in the same direction, but Braudel seems to underestimate the increase in the budgets in relation to the rise in prices. On the basis of his own figures, in the second half of the sixteenth century we find a trebling of the Venetian and Spanish state budgets and a doubling of the French, expressed in real terms; but at the same time the index for Spanish prices, also reckoned in real terms, rose in the same period only from about 80 to about 130.[71]

Did the state's share of the national product in the seventeenth century increase? We shall never possess the statistical information that would permit an exact answer to this question, but we may maintain without any hesitation that the seventeenth century, crisis or no crisis, witnessed an enormous growth in public expenditure. In the seventeenth century the largest armies since the time of the Roman Empire were established,[72] and before the end of the century most of the states also had standing armies in peacetime.[73] Military organization was one of the century's most advanced forms of enterprise:[74] fortresses, navies and royal palaces constituted the century's biggest efforts in planning and organizing and its most precious investments.[75] These facts cannot be left out of account in a discussion of the seventeenth-century crisis. Braudel advocates a 'public vices=private benefit' viewpoint; i.e., state expenditure stimulated the economy as a whole.[76] But this point of view can be correct only if the resources that were used in establishing the armies or in undertaking the year-long campaigns and erecting the fortresses were previously lying idle. If, on the other hand, the resources were transferred from other sectors, there is no reason for surprise when we find that these other sectors manifested symptoms of crisis.

The problem should also be seen from the taxpayer's point of view. That the seventeenth-century wars had a depressive effect on those areas directly involved in the fighting (e.g. the greater part of Germany) is well known, but it is worth emphasizing that there were also countries—like Sweden,[77] Castile[78] or Naples[79]—in the seventeenth century that largely avoided warfare on their own territory but which, on account of the wars, had to carry a heavy tax burden for long periods, causing a decrease in living standards for large sections of the population. Goubert's estimates of a normal budget in Beauvaisis in the second half of the seventeenth century gives an impression of the effect that even a moderate increase in taxes could have. Of the gross product, rent amounted to about 20 per cent; tithes, etc., to about 12 per cent; taxes, etc., to about 20 per cent; and seed and sundry expenses to an average of 20 per cent. For the farmer and his family there remained about 30 per cent of the yield if he was a tenant, about 50 per cent if he owned his own land. According to Goubert's estimates, this distribution of the crop meant that up to three-quarters of the farms in the area investigated were too small to support a family 'Were they, then, condemned to suffer hunger or even to starve to death? The answer is most definitely in the affirmative.'[80] Beauvaisis was still marked by a moderate prosperity right up to

the middle of the 1630s. Retrogression first began around the outbreak of war, the years of crop failure around the middle of the century, 1647–55, being particularly catastrophic. It is difficult to avoid the conclusion that the years of crop failure hit a population that was already taxed to starvation level. That taxation in France at the middle of the seventeenth century increased more rapidly than production is scarcely to be doubted. In the case of Languedoc, Le Roy Ladurie has reckoned the part played by direct taxation in the gross product at a little over 6 per cent in the years approaching 1620, rising to about 13 per cent at the middle of the century. At the same time a corresponding increase in indirect taxation occurred.[81]

We cannot draw any general conclusions on the basis of local investigations, but they illustrate the effect that increased taxation might have on a local level, and the numerous peasant revolts in the France of Richelieu and Mazarin leave no doubt that this fiscal pressure was felt nearly everywhere. Increased taxation pressure in the areas not directly affected by the great wars may be one of the most important causes of the demographic and agrarian crises that hit Europe in the seventeenth century; it may also have contributed towards disruptions within other occupations, either directly—by forcing craftsmen to emigrate and merchants to invest in privileged undertakings such as land, state loans or offices—or indirectly—by accentuating the rural population's competition with the urban industries. Goubert has described the desperate search of the rural population for other means of income,

> 'the incessant search for other forms of income, for piece work and such like, hunting for vacant leases, for wool to spin, for lace to manufacture, for wood to chop, carve or sell, for any small job on the larger estates'.[82]

The pressure on the rural population might be a ruthless, growth-promoting measure by enforcing the utilization of unused manpower reserves or overlooked resources. It is probable that the expansion of the rural industry in suitable areas reflected the need for procuring cash or credit.[83] But the competition from the rural industries mentioned above, which could be observed nearly everywhere in Europe, certainly contributed just as much to the difficulties of the urban industries as the loss of their rural customers.

The economic crisis

This survey of the most important economic sectors indicates that the seventeenth-century crisis was not a universal retrogression, but that it hit the various sectors at different times and to a different extent. The long-term trends in trade and industry are unclear: there were crises at one time or another in every European production centre and in all branches of European trade, but it is impossible to pinpoint a time or a period when European trade and industry as a whole was hit by a depression. On the other hand, the demographic trends and

agricultural prices and production indicate that there was something seriously the matter with the European economy, and the low relative prices combined with the failing yield indicate that we should seek the explanation not solely in poorer climatic conditions or in population pressure—for in that case the prices would have been rising—but in the inability of the population to buy corn and their inability to survive. Finally, if we take a look at the public sector and reckon protection to be a service, in the economic-theoretical meaning of the word, the whole question of a seventeenth-century crisis falls to the ground. Never before was Spain so thoroughly protected as under Philip IV; never before was Germany so thoroughly protected as during the Thirty Years' War; and never before was France so thoroughly protected as under the cardinals and Louis XIV! The production of protection was the seventeenth century's 'leading sector'.

It would be reasonable to suppose that these phenomena were interrelated. An increase in taxation in the widest sense, which exceeded the increase in production in an economy still chiefly based on subsistence agriculture, would have precisely these effects. Part of the population was always living at or near subsistence level, and an increase in the tax burden would reduce their chances of surviving an especially difficult year. Furthermore, it may be regarded as probable that a population would react to a drop in its available income by a reduction in the birth rate, e.g., by raising the age at first marriage. The effect in the agricultural sector would, with the exception of a few privileged localities, be purely negative, as the decrease in private demand would not be compensated for by an increase in public expenditure. For industry and trade the effects would be more complicated. Increased public demand would probably more than compensate for the reduced private demand, but not necessarily within the same production areas. Moreover, the difference in the level of taxes and in the tax systems would have different effects on the production costs and thereby on the ability to compete in the various production centres.

'Taxes are not a *deus ex machina* to explain everything.'[84] On the other hand, every attempt to understand the seventeenth-century economic crisis without taking account of the distribution of income that took place through the public sector is doomed to failure. What appears to be a *'renversement de la tendance majeure'* was in reality the result of an altered pattern of demand precipitated by the transfer of income through taxation. The seventeenthcentury crisis was a distribution crisis, not a production crisis.

This conclusion has methodical implications affecting problems other than the one treated here. The last generation of economic historians has followed the economic theoreticians by interesting themselves more in the production of the goods than in their distribution. But this isolation of the object of investigation, however fruitful it may be in an economic analysis, has turned out to be destructive in an historical investigation. Quantification and the drawing up of series provide sure results within the given framework, but do not permit us to draw up either global fluctuations or quadri- or quinquaecyclic systems.[85]

II
THE POLITICAL CRISIS

Trevor-Roper's 'general crisis'—i.e., one in the relations between State and society—may be left uncontested, for it is so general as to be applicable to every revolt. On the other hand, as already pointed out by several of the contributors to the symposium on Trevor-Rope's thesis in 1960,[86] the pair of concepts, court-country, has scarcely any European validity. The dualism between a parasitic bureaucracy and an indignant, puritanically minded country opposition does not explain the revolts in the middle of the seventeenth century, which formed the starting-point of Trevor-Roper's discussion. The revolts were by no means directed against a stagnating parasitism, but against a dynamic absolutism which, with its taxation policy, violated the customary laws and threatened to disrupt the social balance or deprive parts of the population of their livelihood. In Catalonia and Portugal the revolts were precipitated not by dissatisfaction with the established order, but by dissatisfaction with Olivares's attempt to alter the established order when he demanded that the vice-royalties should contribute towards the costs of Spain's foreign policy side by side with Castile.[87] The revolt in Naples followed after a number of years of large contributions to the Spanish war chest, which not only had been economically devastating, but also had created chaos in the traditional distribution of authority and wealth.[88] The revolt in Palermo took place under the slogan, 'Long live the King and down with the taxes', a slogan that is to be found time and time again during the revolts of the French peasants.[89] The opposition of the Parlement of Paris in the 1640s had no ideological aim, but was concentrated against the Crown's fiscal legislation; and the Fronde of the Parlement was triggered by a legislation that would have decreased the Parlement's own privileges.[90] In England the trends are less clear, but even in this case there is an apparent conflict between the monarchy's attempt to strengthen its economic independence and the taxpayer's defence of his customary rights. Even in the *coup d'état* in the Netherlands the fiscal element is present, though in this case the conflict was precipitated by the states of the province of Holland refusing to continue payment of the soldiers they had to maintain, which were under the command of the Stadtholder.[91]

The common factor in the contemporaneous revolutions is thus something far less subtle than Trevor-Roper's dualism between court and country. We do not need to look for abstract similarities between the social structures of the societies in revolt, for there is a concrete similarity between the policy of the governments concerned, that is in their attempts to increase their income or to secure control over the state revenue regardless of customary rights. This statement, which is supported both by our knowledge of the revolts and by the analysis of the economic crisis presented above, can be further substantiated if we extend the comparative investigation to include those countries that did not suffer internal armed conflict in the middle of the seventeenth century. In Bavaria the power of the Estates was broken during the Thirty Years' War; their attempts to regain

control of taxation after 1648 led to no result.[92] In the Hohenzollern possessions the electoral independence was achieved step by step between 1653 and 1667; when the conflict with the Estates' institutions and towns was over, the Elector had made sure of a regular annual income which enabled him to maintain a standing army.[93] In 1655 in Hessen-Kassel, the Landgrave out-manoeuvred the Estates, thereby obtaining a standing army, which made him and his successors independent of Estates grants.[94] In Saxony and Württemberg we find the same problem again, but without any clear victory for either the Prince or the Estates.[95] In Denmark the formal introduction of the absolute monarchy in 1660 was coupled with the financial crisis following the Northern War, and it was succeeded by a comprehensive modernization of finance and administration.[96] In Sweden a battle over the state finances started in 1655 and stretched over several decades, but the final result was once again a strengthening of the monarchy's fiscal position.[97]

All this is well known; but it is necessary to emphasize it at this point in order to show that Trevor-Roper started in the wrong place in taking for his point of departure the revolts. That it came to armed conflict in some states is not a valid criterion; the chosen starting-point should be the conflict, be it armed or unarmed, that is common to all the states. Behind the conflict we find the same thing everywhere: the state's demand for higher revenues. In some cases the tax demands were coupled with financial reforms that were not necessarily unfair, but which undermined customary rights; in other cases the increased burden of taxation came to rest on the population groups already living below the bread line. The different reactions in different countries, regardless of whether or not it came to armed conflict, or whether the protests led to any results, depended on the social and economic situation of the country in question and on the policy chosen by the governments (not least upon the choice of the social groups with which they chose to cooperate and the social groups upon which they chose to lay the burden of the increased taxation). But in every case it was the governments that acted in a revolutionary manner: the tax demands disrupted the social balance. They did not create a revolutionary situation: they were in themselves a revolution.[98] The six contemporaneous revolutions can only be seen as one if we rechristen them 'the six contemporaneous reactions'.

III
THE PROBLEM OF ABSOLUTISM

Behind the symptoms of economic crisis, and behind the internal conflicts in the European countries in the middle of the seventeenth-century, we find the same factor: the growth of state power and the increased fiscal demands. The problem of the crisis is therefore the problem of absolutism.

Naturally the observation of a connection between the taxation pressure and the revolts is not original—it is an interpretation that in many respects approaches Mousnier's view of the seventeenth-century crisis. Mousnier has pointed out that

increased fiscal demands hit all groups of society, and in his later writings he has pointed out repeatedly and with ever-increasing emphasis the decisive importance of taxation pressure for the peasant revolts in seventeenth-century France. In *Peasant Uprisings* he says, concerning the taxes: 'The lists that can be made from the records of these impositions on the people are still capable of causing the scholar who draws them up to clench his fists, however *blasé* about poverty and wretchedness he may be.'[99] Nevertheless, he undertakes a defence of absolutism, at least where France is concerned, which blocks the way for further analysis. The state for Mousnier is part of the fight against the crisis, not its initial cause. It is the guarantee of order and progress, of freedom itself, and absolutism is a fulfilment of the people's wishes: 'Absolutism was the wish of the masses who saw their salvation through concentrating all powers in the hands of one man.'[100] 'France adopted the goal of saving European liberties from the Habsburg claims of a universal monarchy.'[101] 'French governments of the seventeenth century were governments of war, economic stagnation and social unrest. They thus grew increasingly dictatorial.'[102] Even in *Peasant Uprisings*, in which Mousnier particularly emphasizes the importance of taxation pressure for popular revolts, pointing out that it was the State and not the fighting populace that violated the customary rights, he defends the Government's foreign policy as being a political necessity: the wars were national and France had long frontiers to defend.[103] The wars are treated in this book as a *circonstance*, something external, on a par with climatic changes and international price conditions.

The importance of taxation pressure in generating the French peasant revolts has been just as strongly emphasized by the Soviet historian Porshnev. But whereas the wars, for Mousnier, are an unavoidable calamity and the governments the defenceless victims of pressures within the international system, Porshnev regards the wars as a governmental red herring concealing their true function: the subjugation of the exploited classes.

> During this period, French absolutism pursued four vital aims. In descending order of importance, they were, firstly and above all, the subjection of the exploited classes… Then, to induce a substantial part of the bourgeoisie to support the state in the conflict,…thirdly, to ensure a centralised income for the bulk of the nobility, via taxes… finally, the protection, and, if possible, the extension of the borders of France.[104]

Or, expressed more briefly in another context, 'Absolutism was an institution for the repression and oppression of the peasantry in the interests of the nobility.'[105] But this identification of the ruling class with the nobility is either meaningless or incorrect. It is incorrect if we postulate a continuity between the ruling class before and after the victory of absolutism for, as both Porshnev's own and Mousnier's investigations have proved, the dividing line in the internal French conflict of the seventeenth century did not run between the classes, but within

them. As regards both recruiting and function, the nobility that flocked around Louis XIV was a different group from the nobility that had fought in the Wars of Religion. It is meaningless to define the nobility merely as the class that exploited the peasants—there was not much else to exploit; even with Porshnev's own theoretical background the interest must lie in the actual *nature* of the exploitation during the later stages of feudalism.

It is scarcely accidental that the two collections of Chancellor Séguier's papers that finally landed in Leningrad and Paris, respectively, have been able to lead two eminent historians to opposite conclusions. By virtue of their professional scepticism, most historians would scarcely be able to avoid the iconoclastic thought that Mousnier and Porshnev are fighting out the twentieth-century ideological battle on seventeenth-century ground. We can accept Mousnier's conclusions only if we believe with him that the nation is more than a sum of its individuals, that the masses could have a common wish and 'France' could have a goal. We can accept Porshnev's conclusions only if we believe with him that the government was a tool in the service of one particular class, and that the most radical division between the classes in seventeenth-century France lay between the nobility, the bourgeoisie and the peasants. But the more we know about the seventeenth century—not least through the publication of the two antagonists' own research—the more difficult it becomes to believe that the social grouping that came into being at the close of the eighteenth century was identical with that of the seventeenth century.

It is impossible to harmonize these two viewpoints, but perhaps it is not really necessary to make a choice, for behind both of them lies the idea of the government as an institution that acts rationally, in the interests either of the nation or of a social class. One might invoke the support of political science against such a simplified conception of governmental functions, but it would be more appropriate to seek a more subtle approach among those historians who have been concerned with the European *ancien régime*. J.H.Elliott [see Chapter 5, pp. 112–15] has criticized the use modern historians have made of the concept of revolution where the seventeenth century is concerned. He pointed out that the French Revolution has become, as it were, a paradigm for revolution as a whole, and that historians have even attempted to analyse older revolts and upheavals in accordance with this model. But, he says,

> it is open to question whether our persistent search for 'underlying social causes' has not led us down blind alleys… Political disagreement may, after all, be no more and no less than political disagreement —a dispute about the control and exercise of power.

Perhaps historians are just as dependent on the nineteenth century for their view of governments as for their view of revolutions, though with the difference that, as far as governments are concerned, there are not one but two models available —the 'conservative national' and the 'conservative sociological'. A third

possibility, which might constitute the first step towards an emancipation from these models, has been outlined by Lane and van Klaveren, but has apparently gone unnoticed by those historians concerned with the seventeenth century—perhaps because their theories are too destructive of well-loved myths both on the Right and on the Left.

For Lane, governments are to be regarded as institutions specializing in the organized exercise of violence; they may be regarded as producers of a particular service, that of protection. Normally the production of protection will yield a profit over and above the costs of production—the producer has a natural monopoly—and a policy that aims at reducing production costs and the sales price is unlikely to be encountered unless the government concerned is controlled by the customers, which—as Lane points out—is an historical exception.[106] Independently of Lane, though on somewhat similar lines, van Klaveren has investigated the part played by corruption in the pre-industrial socio-economic system. Government officials used their power and influence as a matter of course to enrich themselves at the expense of the prince or the public. Corruption during the *ancien régime* was not a criminal action, but a part of the constitution.[107]

It is clear that this model does not exclude the two models characterized above as the 'conservative national' and the 'conservative sociological'. The government may act on behalf of all consumers of protection, i.e., for the nation, or in the interests of a single social class; but both these possibilities must be regarded as extreme cases. By bringing the actual wielders of power into focus, and by presuming that they act primarily in their own interest, what may be described as the Lane-van Klaveren model opens up very important analytical possibilities. As a consequence of their outlook it will be possible to extract the question of absolutism from its sterile deadlock and to deepen our understanding of it as a political system.

IV
CONCLUSION

In this Chapter I have attempted to review the concept of the seventeenthcentury crisis in the light of our present knowledge of that century's economy and of internal political conflicts in the middle of the century. Both from an economic and from a political point of view, the tracks pointed in the same direction; those symptoms of crisis that may be demonstrated led to an already well-known phenomenon: the growing power of the state, frequently characterized by the introduction of absolutism. The crisis was not a production crisis but a distribution crisis; the revolts were not social revolutionary, but reactionary against the demands of the state. According to preference, we can reject the concept of crisis altogether or couple it with the problem of absolutism. But this very coupling of the two problems seems to provide possibilities for a fruitful resumption of the discussion. If the governmental actions were revolutionary and

the revolts reactionary, if we are to seek the dynamic factor in conjunction with the State and not with the people, we must abandon the stereotype conception of absolutism as a passive instrument for the nation or a class, and resume the analysis of early modern monarchy as a political system, on the assumption that governments were not only products of that society in which they arose, but were also instrumental by means of their policy—i.e. in the choice of whom they taxed and whom they subsidized—in forming that society.

NOTES

* Originally published as 'Det syttende Arhundredes Krise' in *Historisk Tidsskrift* (Dansk), XII (1970), pp. 475–504. Translated by Paūla Hostrūp-Jessen.

1 P.Chaunu, 'Le renversement de la tendance majeure des prix et des activités au XVIIe siècle', *Studi in onore di Amintore Fanfani*, IV (Milan, 1962); and 'Réflections sur le tournant des années 1630–1650', *Cahiers d'Histoire*, XII (1967); R. Romano, 'Tra XVI e XVII secolo. Una crisi economica: 1619–1622' (Chapter 7 in this volume); and 'Encore la crise de 1619–1622', *Annales E.S.C.*, XIX (1964).

2 H.R.Trevor-Roper, 'The general crisis of the seventeenth century', *Past and Present*, XVI (1959), reprinted in Trevor Aston (ed.), *Crisis in Europe, 1560–1660* (London, 1965).

3 E.Hobsbawm, 'The crisis of the seventeenth century', *Past and Present*, V–VI (1954), reprinted in Aston, op. cit.

4 Hobsbawm's thesis has been sharply and expertly criticized within his own theoretical framework in A.D.Lublinskaya, *French Absolutism: The Crucial Phase, 1620–29* (Cambridge, 1968), pp. 38–75.

5 R.Mousnier, *Les XVIe et XVIIe siècles*, 3rd edn (Paris, 1961), p. 159.

6 A.E.Lublinskaya, *French Absolutism;* Ivo Schöffer, 'Did Holland's Golden Age coincide with a period of crisis?', (see Chapter 4 in this volume, pp. 87–107).

7 Romano, 'Encore la crise', p. 37.

8 Such important series as John U.Nef's estimates of the extent of the English coal production in *The Rise of the British Coal Industry* (London, 1932), I, pp. 19–21 and II, pp. 379–89, and Ralph Davis's estimates of the English merchant navy in *The Rise of the English Shipping Industry* (London, 1962) pp. 7–15, which show considerable advance throughout the seventeenth century, have been ignored, whereas far less important series, such as the soap production in Amsterdam, have been taken up for discussion. Another example is the argumentation based upon the number of ships sent out to Asia from one single country—see my 'European shipping to Asia, 1497–1700', *Scandinavian Economic History Review*, XVIII (1970).

9 Ivo Schöffer, 'De demografie van het oude Europa', *Tijdschrift voor Geschiedenis*, LXXIV (1961), pp. 1–31; K.J.Beloch, *Bevölkerungsgeschichte Italiens* (3 vols, Berlin, 1937–61), particularly vol. III pp. 350–4; Carlo M.Cipolla, 'Four centuries of Italian demographic development', *Population in History*, D.V.Glass and D.E. C. Eversley, eds (London, 1965), p. 573; Günther Franz, *Der Dreißigjährige Krieg und das Deutsche Volk*, 3rd edn (Stuttgart, 1961). Uncertainty is greatest where Castile is concerned, but it is most probable that the population figures reached

their peak before the plague of 1599–1600; E.J.Hamilton, 'The decline of Spain', *Economic History Review*, 1st series, VIII (1937–8) J.H.Elliott, 'The decline of Spain', *Past and Present*, XX (1961), reprinted in Aston, op. cit.; Hermann Kellenbenz, 'The impact of growth on government: the example of Spain', *Journal of Economic History*, XXVII (1967); J.Vicens Vives (ed.), *Historia Social y Económica de España y América* (Barcelona, 1957), pp. 251–5.

10 J.Nadal and G.Giralt, *La Population Catalane de 1522 à 1717* (Paris, 1967), pp. 19–23; according to P.Vilar, *La Catalogne dans l'Espagne Moderne* (Paris), I, pp. 617–20, 635, moderate growth continued throughout the century, only interrupted by the plague of 1650–3.

11 J.A.Faber *et al.*, 'Population changes and economic developments in the Netherlands: a historical survey', *A.A.G.Bijdragen*, XII (1965); R.Mols, 'Die Bevölkerungsgeschichte Belgiens im Lichte der heutigen Forschung', *Vierteljahrschrift für Sozial- und Wirtschaftgeschichte*, XLVI (1959).

12 S.Hoszowski, 'The Polish Baltic Trade in the 15th–18th centuries', *Poland at the 11th International Congress of Historical Sciences in Stockholm* (Warsaw, 1960), p. 119; Aksel Lassen, *Fald og Fremgang* (Aarhus, 1965).

13 E.A.Wrigley, 'Family limitation in pre-industrial England', *Economic History Review*, XIX (1966).

14 Denis Richet, 'Croissance et blocage en France du XVe au XVIIIe siècle', *Annales E.S.C.*, XXIII (1968); P.Goubert, 'Recent theories and research in French population between 1500 and 1700', *Population in History*, op. cit., p. 472.

15 P.Chaunu, *La Civilisation de l'Europe Classique* (Paris, 1966), pp. 254–5.

16 Wrigley, op. cit., pp. 86ff. Chaunu, op. cit., pp. 189ff.

17 Chaunu, op. cit.

18 P.Goubert, 'Disparités de l'ancienne France rurale', *Cahiers d'Histoire*, XII (1967).

19 Wrigley, op. cit., p. 103.

20 E.A.Wrigley, 'A simple model of London's importance in changing English society and economy, 1650–1750', *Past and Present*, XXXVI (1967), p. 98.

21 Wilhelm Abel, *Agrarkrisen und Agrarkonjunktur*, 2nd edn (Stuttgart, 1966).

22 See the survey by F.Braudel and F.C.Spooner in *Cambridge Economic History of Europe*, IV (Cambridge, 1967), Chapter vii.

23 Richet, op. cit., pp. 760–3.

24 Abel, op. cit, p. 143.

25 B.H.Slicher van Bath, *The Agrarian History of Western Europe, A.D. 500–1850* (London, 1963); 'De oogstopbrengsten van verschillende gewassen, voormelijk graanen, in verhouding tot het zaazaaid, ca. 810–1820', *A.A.G.Bijdragen*, IX (1963); 'Yield ratios 810–1820', ibid., 10 (1963); 'Les problèmes fondamentaux de la société pré-industrielle en Europe occidentale', ibid., XII (1965) and 'Die europäischen Agrarverhältnisse im 17. und der ersten Hälfte des 18. Jahrhunderts', ibid., XIII (1965).

26 B.H.Slicher van Bath, 'Oogstopbrengsten', pp. 74–80.

27 Cf., e.g., Fridlev Skrubbeltrang in (Danish) *Historisk Tidsskrift*, XII (1964), pp. 391–4.

28 Slicher van Bath, 'Agrarverhältnisse', p. 145.

29 Aksel E.Christensen, *Dutch Trade to the Baltic about 1600* (Copenhagen, 1941), p. 420 and diagrams xvi-xviii.

30 J.A.Faber, 'The decline of the Baltic grain trade in the second half of the seventeenth century', *Acta Historiae Neerlandica*, I (1966); P.Jeannin, 'Les comptes du Sund comme source pour la construction d'indices généraux de l'activité économique de l'Europe', *Revue Historique*, CCXXXI (1964); cf., S. Hoszowski, 'The Polish Baltic trade in the 15th–18th centuries', *Poland at the 11th International Congress of Historical Sciences in Stockholm* (Warsaw, 1960) and A.Soom, *Der Baltische Getreidehandel im XVII Jahrhundert* (Stockholm, 1961).

31 Faber, op. cit., p. 131; J.Jaquand, 'La production agricole dans la France du XVII siècle', *Dix-Septième Siècle*, LXX (1966), p. 32.

32 C.Wilson, *England's Apprenticeship, 1603–1763* (London, 1965), pp. 146ff.; R. Davis, 'English Foreign Trade, 1660–1700', *Economic History Review*, 2nd series, VIII (1954), reprinted in W.E.Minchinton (ed.), *The Growth of English Overseas Trade* (London, 1969).

33 Carlo M.Cipolla, 'The decline of Italy: the case of a fully matured economy', *Economic History Review*, 2nd series, V (1952); Domenico Sella, 'Les mouvements longs de l'industrie lainière à Venise', *Annales E.S.C.*, XII (1957). Revised versions of both these articles are reprinted in Brian Pullan (ed.), *Crisis and Change in the Venetian Economy* (London, 1968); Domenico Sella, *Commerci e Industrie a Venezia nel Secolo XVII* (Venice and Rome, 1961); R.Romano, 'A Florence au XVIIe siècle', *Annales E.S.C.*, VII (1952).

34 Hermann Kellenbenz, 'The impact of growth on government: the example of Spain', *Journal of Economic History*, XXVII (1967), pp. 356–7; Felipe Ruiz Martin, *Lettres Marchandes Echangées entre Florence et Medina del Campo* (Paris, 1965), pp.cviii–cix.

35 Vilar, *La Catalogne dans l'Espagne moderne*, op. cit., I, pp. 593–4.

36 C.Wilson, 'Cloth production and international competition in the seventeenth century', *Economic History Review*, 2nd series, XIII (1960); F.J.Fisher, 'London's export trade in the early seventeenth century', *Economic History Review*, 2nd series, III (1950); Davis, 'English foreign trade'.

37 N.W.Posthumus, *De Geschiedenis van de Leidsche Lakenindustrie*, III (Leiden, 1939), pp. 924–7 and table 114, p. 941; Wilson, 'Cloth production', p. 214. R. Romano interprets Posthumus in an entirely opposite way, maintaining that a reduction in quality occurred from the 1620s; R.Romano, 'Tra XVI e XVII secolo', Chapter 7, p. 174 below, referring to Posthumus, op. cit., pp. 941–6. This, however, must be the result of a misreading. It is correct that the manufacturers of the cheaper qualities tried to meet the competition by a further reduction in quality, but of far greater importance was the expansion in the production of the more expensive cloth (especially in the years around 1635–55) and mohair (especially after 1650).

38 B.E.Supple, *Commercial Crisis and Change in England, 1600–1642* (Cambridge, 1959), p. 258.

39 Ibid., pp. 149–52.

40 W.E.Minchinton in his introduction to *The Growth of English Overseas Trade*, op. cit, p. 9; Fisher, op. cit., reprinted in Minchinton, op. cit., pp. 66–8.

41 R.W.K.Hinton, *The Eastland Trade and the Common Weal* (Cambridge, 1959), pp. 226–9.

42 Wilson, 'Cloth production', pp. 217–21.

43 Pierre Deyon and A.Lottin, 'Evolution de la production textile à Lille aux XVIe et XVIIe siècles', *Revue du Nord*, XLIX (1967).
44 E.Coornaert, *La Draperie-sayetterie d'Hondschoote* (Paris, 1930), pp. 48–9, 56–7, 493–5.
45 Jan Craeybeckx, 'Les industries d'exportation dans les villes flamandes au XVII siécle, particulièrement à Gand et à Bruges', *Studi in onore di Amintore Fanfani*, IV (Milan, 1962).
46 Pierre Deyon, 'Variations de la production textile aux XVIe et XVIIe siècles: sources et premiers résultats', *Annales E.S.C.*, XVIII (1963); 'La production manufacturière en France au XVIIe siècle et ses problèmes', *Dix-Septième siècle*, LXX (1966), pp. 53–6 and *Amiens: Capitale provinciale* (Paris and The Hague, 1967), p. 170; P.Goubert, *Beauvais et le Beauvaisis de 1600 à 1730* (Paris, 1960), pp. 585–92.
47 Herbert Kisch, 'Growth deterrents of a medieval heritage: the Aachen woollen trades before 1790', *Journal of Economic History*, XXIV (1964), pp. 524ff.
48 Hinton, op. cit., pp. 104 and 156; Supple, op. cit., pp. 137–42.
49 Hermann Kellenbenz, 'Industries rurales en Occident de la fin du moyen âge au XVIIIe siècle', *Annales E.S.C.*, XVIII (1963), pp. 877–8; E.L.Jones, 'Agricultural origins of industry', *Past and Present*, XL (1968); Slicher van Bath, 'Agrarverhältnisse', p. 147; Vilar, op. cit., I, pp. 596ff.; D.Sella, 'Industrial production in seventeenth century Italy', *Explorations in Entrepreneurial History*, 2nd series, VI (1969), pp. 236–8; Deyon, *Amiens*, pp. 206–10.
50 P.Chaunu, *Séville et l'Atlantique* (1504–1650), VIII[2:1] (Paris, 1959 edn), pp. 15ff.
51 E.J.Hamilton, *American Treasure and the Price Revolution in Spain 1501–1650* (Cambridge, Mass., 1934), p. 34.
52 Michel Morineau, 'D'Amsterdam à Séville: de quelle réalité l'histoire des prix estelle le miroir?', *Annales E.S.C.*, XXIII (1968), p. 196. Already from 1656–60 to 1661–5 imports rose from 3.4 million *pesos de mina* to 28.8 million *pesos de mina*. The record from 1591–5, 35.2 million pesos, was exceeded in 1676–80, 1686–90 and 1696–1700. In no five-year period after 1661 was the import less than 22 million *pesos de mina*.
53 Wilson, *England's Apprenticeship*, pp. 56ff. Davis, *The Rise of the English Shipping Industry*, Chapter 13.
54 Christensen, op. cit., p. 358, cf., diagram xiii
55 P.Jeannin, 'Les comptes du Sund comme source pour la construction d'indices généraux de l'activité économique en Europe', *Revue Historique*, CCXXXI (1964).
56 Hinton, op. cit., pp. 105–12.
57 Jeannin, op. cit., pp. 337, 340.
58 Romano Chapter 7, pp. 153–205, refers to the Dutch figures; Chaunu, 'Le renversement de la tendance majeure', refers to the Portuguese figures. The series selected in both cases support the chronology preferred by the author.
59 Niels Steensgaard, *The Asian Trade Revolution of the Seventeenth Century* (Chicago and London, 1974), pp. 169–74.
60 Francis Bacon's classic formulation, 'the increase of any estate must be upon the foreigner (for whatsoever is somewhere gotten is somewhere lost)', *Essays* (London, 1962 edn), p. 45, expresses a train of thought common to most Mercantilist theoreticians and statesmen; cf., E.F.Heckscher, *Merkantilismen*, 2nd edn, II (Stockholm, 1953), pp. 18–22.

61 In Chapter 4, pp. 87–107, Ivo Schöffer criticizes the concept of a seventeenthcentury crisis from this point of view.
62 It is probable that the definite decline of The Netherlands should be placed even later: C.Wilson, 'The economic decline of the Netherlands', *Economic History Review,* 1st series, IX (1939), pp. 111–27.
63 Davis, 'English foreign trade'.
64 J.Delumeau, 'Le commerce extérieur français au XVIIe siècle', *Dix-septième Siècle,* LXX (1966); also P.Goubert, *Louis XIV et vingt millions de Français* (Paris, 1966), pp. 141f.
65 Jorgen H.P.Barfod, *Danmark-Norges Handelsflåde, 1650–1700* Helsingór, (1967).
66 Strangely enough, it is the economic historians who are most liable to overlook the economic importance of the public sector. Is this the result of the influence of an economic science that has preferred to dwell on the production of goods rather than on their distribution? Cf., Gunnar Myrdal, *Economic Theory and Underdeveloped Regions* (London, 1963), pp. 115–16. The most paradoxical is Chaunu's attitude. He maintains that there was a far more rapid growth in government revenue than in production in the seventeenth century, and adds, 'de là à affirmer qu'il est le moteur de la croissance il n'y a qu'un pas à franchir prudemment': *La Civilisation de l'Europe Classique,* p. 59. But out of the more than 600 pages in this book only half a page is used for a discussion of the finances, and Chaunu's numerous articles dealing with the short- and long-term fluctuations of the sixteenth and seventeenth centuries may be searched in vain for a reference to the public sector. The interrelation between fiscalism and popular revolts has been emphasized most strongly by R.Mousnier, e.g. in his *Peasant Uprisings* (London, 1972), pp. 305ff., and by B.Porshnev in *Les Soulèvements populaires en France de 1623 à 1648* (Paris, 1963).
67 Of course there is also the opposite viewpoint that the militarist policy created the demand, which formed the basis for the development of the modern economy: Werner Sombart, *Krieg und Kapitalismus* (Munich, 1913), John U.Nef, in *War and Human Progress* (Cambridge, Mass., 1950), has disputed Sombart's viewpoint, but apart from this one case the problem seems to have vanished from the field of recent historical research. A French economist, Gaston Imbert, has indicated the possibility of a connection between a pre-industrial Kondratieff cycle and the wars: *Des Mouvements de longue durée Kondratieff* (Aix-en-Provence, 1959), pp. 394–8, 420–4.
68 F.Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II,* I (London, 1972), p. 451.
69 Antonio Domínguez Ortiz, *Política y Hacienda de Felipe IV* (Madrid, 1960), pp. 180–5.
70 Braudel, op. cit., II, pp. 684–6.
71 Ibid., p. 33.
72 Quincy Wright, A *Study of War,* 2nd edn (Chicago, 1965), pp. 232–7, and appendix XXI table 49; G.Parker, 'The "military revolution 1560–1660"—a myth?', *Journal of Modern History,* XLVIII (1976), pp. 195–214.
73 Hans Delbrück, *Geschichte der Kriegskunst im Rahmen der Politischen Geschichte,* IV (Berlin, 1920), pp. 255–7.
74 Fritz Redlich, *The German Military Enterpriser and his Work Force* (Wiesbaden, 1965).

75 The royal palaces (constructions, etc.) were entered at well over 15 million livres on the debit side of the French budget for 1685: Werner Sombart, *Luxus und Kapital-ismus* (Munich, 1913). We may compare this amount with the share capital of the French East India Company after its reconstruction the same year, 1.6 million livres: Paul Kaeppelin, *La Compagnie des Indes Orientales et François Martin* (Paris, 1908), p. 95.
76 Braudel, op cit., I, pp. 409–10.
77 Eli F.Heckscher, *Sveriges Ekonomiska Historia,* I^2 (1950), pp. 420–2.
78 J.H.Elliott, 'The decline of Spain', in Aston, op. cit., pp. 182f.; op. cit., pp. 176ff.
79 R.Viflari, *La Rivolta antispagnola la Napoli, le origini (1585–1647)* (Bari, 1967), pp. 6–7.
80 Goubert, *Beauvais et le Beauvaisis,* pp. 180–2 and 'The French peasantry of the seventeenth century: a regional example', *Past and Present,* X (1956), reprinted in Aston, op. cit., p. 156 cf., however, the more conservative estimate in his *L'Ancien Régime,* I (Paris, 1969), pp. 100, 119–30.
81 E.Le Roy Ladurie, *Les Paysans de Languedoc* (Paris, 1966), pp. 294, 481–2.
82 Goubert, 'The French peasantry', p. 157; cf., *Beauvais et le Beauvaisis,* pp. 131f.
83 Kellenbenz, 'Industries rurales en Occident', pp. 877f.
84 C.Wilson, 'Taxation and the decline of empires, an unfashionable theme', *Bijdragen en Mededelingen van het Historisch Genootschap,* LXXVII (1963).
85 The expression is taken from P.Chaunu, 'Le XVIIe siècle, problèmes de conjoncture, conjoncture globale et conjonctures rurales françaises', *Mélanges Antony Babel* (Génève, 1963), p. 354. For a more detailed criticism of the theory of cyclic fluctuations especially championed by Chaunu see my 'Det syttende arhundredes krise' (Danish), *Historisk Tidsskrift,* XII (1970), pp. 488–92.
86 'Trevor-Roper's "general crisis", symposium', *Past and Present,* XVIII (1960), reprinted in part in Aston, op. cit.
87 J.H.Elliott, *The Revolt of the Catalans* (Cambridge, 1963), pp. 512–15 and *passim.*
88 Villari, *La Rivolta Antispagnola,* pp. 124–38, 144–9.
89 H.G.Koenigsberger, 'The revolt of Palermo in 1647', *Cambridge Historical Journal,* VIII (1944–6), p. 133; Porchnev, *Les Soulèvements Populaires en France.*
90 E.H.Kossmann, *La Fronde* (Leiden, 1954), Chapter 2.
91 Pieter Geyl, *The Netherlands in the Seventeenth Century,* II (London, 1964), pp. 15–17; *Algemene Geschiedenis der Nederlanden,* VII (Utrecht, 1954), pp. 4–7.
92 F.L.Carsten, *Princes and Parliaments in Germany* (Oxford, 1959), pp. 400ff.
93 F.L.Carsten, *The Origins of Prussia* (Oxford, 1964), pp. 185ff., 207ff., 243ff.
94 Carsten, *Princes and Parliaments,* pp. 182ff.
95 Ibid., pp. 72ff., 228ff.
96 Johan Jørgensen, 'Bilantz 1660, Adelsvaeldens bo', *Festskrift til Astrid Friis* (Copenhagen, 1963), pp. 153ff.; C.O.Bøggild-Andersen, *Hannibal Sehested, En Dansk Statsmand,* II (Aarhus, 1970), pp. 57ff.
97 Hans Landberg, 'Kungamaktens emancipation. Statsreglering och militärorganisation under Karl X Gustav och Karl XI', *Scandia,* XXXV (1969).
98 At the 13th International Congress of Historical Sciences in Moscow, 1970, discussion of the viewpoints put forward here revealed a fundamental disagreement concerning the definition of the concept of revolution. In order to prevent misunderstandings I wish to make it clear that I define a revolution as every

comprehensive alteration of custom and law put into effect by violent means, or under threat of violence.

99 Mousnier, *Peasant Uprisings*, p. 307.
100 Mousnier, *Les XVIe et XVIIe Siècles*, p. 245.
101 Ibid., p. 281.
102 R.Mousnier, 'La participation des gouvernés à l'activité des gouvernants dans la France de XVIIe et XVIIIe siècles', *Etudes Suisses d'Histoire Générale*, XX (1962–3), p. 216.
103 Mousnier, *Peasant Uprisings*, pp. 308–9.
104 Porshnev, *Les Soulèvements Populaires*, p. 458.
105 B.Porshnev in *XIIe Congrès International des Sciences Historiques*, V, Actes (Vienna, 1965), p. 679.
106 F.C.Lane, 'Economic consequences of organized violence', *Journal of Economic History*, XVII (1958), reprinted in *Venice and History, the Collected Papers of Frederic C.Lane* (Baltimore, 1966).
107 J.van Klaveren, 'Die historische Erscheinung der Korruption', *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, XLIV (1957) and XLV (1958); and 'Fiskalismus, Merkantilismus, Korruption. Drei Aspekte der Finanz- und Wirtschaftspolitik während des Ancien Régime', ibid., XLVII (1960).

[*Editors' note:* Many of the series of economic data discussed in this article are available in G.Parker and C.H.Wilson (eds), *Introduction to the Sources of European Economic History, 1500–1800* (London, 1977).]

3
GERMANY AND THE SEVENTEENTH-CENTURY CRISIS*

Sheilagh Ogilvie

I
INTRODUCTION

The political, economic, and social upheavals of the mid-seventeenth century are called a 'General Crisis', and debated under that rubric, in most countries.[1] The exception is Germany. For historians of Germany, the Thirty Years' War is the central concept for organizing the seventeenth century.[2] Conversely, historians of the general crisis have largely ignored Germany.[3] This is unsatisfactory on both sides. For the crisis historians it is unacceptable not only because any theory of general crisis must be able to account for Germany, but also because the Thirty Years' War was the most spectacular disorder of the crisis period. German historians, on the other hand, cannot be satisfied with a purely German account of the Thirty Years' War that suppresses the wider political and economic context of what was, after all, a 'European civil war'.[4]

This chapter will reconcile these viewpoints by putting Germany squarely at the centre of a theory of the crisis that takes existing crisis theories as its starting-point, but also shows how the Thirty Years' War, largely caused by the peculiar institutional structure of the Holy Roman Empire, in turn wrought significant institutional change, not just in Germany, but throughout Europe.

II
THE GENERAL CRISIS DEBATE

Lively debate has surrounded the seventeenth-century crisis since the 1950s. One current has argued that the crisis was economic in origin. A second has focused on politics, particularly the mid-century revolts and rebellions. A third current has adopted a sceptical stance towards the very concept of a general crisis.

The economic strand of the debate, in turn, falls into three broad classes: Marxist theories, theories stemming from price historians, and theories that locate the cause of the crisis in long-term ecological or demographic movements.

The Marxist historians of the general crisis saw it as a critical juncture in the transition from feudalism to capitalism. E.Hobsbawm opened the debate in 1954

along these lines,[5] followed by B.Porshnev, who saw two separate stages of class struggle. The mid-century revolts were a 'lower form of class struggle' between peasants and feudal nobility, which the nobility won by developing the central state. A 'higher' class struggle for control of that new entity then followed, between bourgeoisie and feudal aristocracy.[6] Variants on these themes followed: M.Hroch and J.Petrán argued that an exogenous contraction in demand triggered the crisis, issuing in rebellions in the west and feudalization in the east.[7] Class struggle, in other versions, is less important than changes in the forces and relations of production.[8]

All Marxist theories shared two basic elements: the revolts were class conflicts, and the crisis was one of production, part of the transition from feudalism to capitalism.[9] But there was wide disagreement. Was the primary cause of the crisis changes in production, or in the balance of class power? Which classes were in conflict? Was this the first crisis of rising capitalism or the last crisis of declining feudalism?[10]

Another group of economic historians, less theoretically motivated than the Marxists, concentrated on the evidence of price trends, diagnosing the seventeenth century as a period of instability following the sixteenth-century 'price revolution'. P.Chaunu focused on the evidence of Spanish-American trade, whose final two Kondratieff cycles (the violent fluctuations of 1600–20, and the general decline of 1620–50) he identified with the general crisis, possibly as its cause.[11] R.Romano broadened the focus to the whole European economy, and to agriculture rather than trade as that economy's largest sector. While underlying structural factors such as the reimposition of serfdom play a role in Romano's theory, a Keynesian demand slump is viewed as the trigger, associated with the monetary upheavals of northern and central Europe in 1619–22.[12]

Finally, a third group of economic theories of the crisis focused on ecological and demographic trends. J.A.Eddy argued that the 'Maunder minimum', the period of low sunspot activity in the seventeenth century, caused lower temperatures and higher precipitation, poorer and later harvests, and a pan-European (even world-wide) subsistence crisis.[13] E.Le Roy Ladurie argued that the cycles of expansion and contraction in the European economy were caused by an inescapable Malthusian dynamic. When populations hit their production ceilings, they limited their numbers by the 'positive checks' of famine, pestilence and war and the 'preventive checks' of late marriage and low fertility.[14] According to Le Roy Ladurie, Europe reached such a Malthusian ceiling in the seventeenth century, resulting in a protracted crisis of subsistence.[15]

The political theories of the crisis have as a rule been less sophisticated and less general than those put forward by economic historians. While even contemporaries were aware of the revolts throughout Europe in the mid-seventeenth century,[16] it took until the mid-twentieth century for historians to overcome their nationalist isolation and tell Europe-wide stories about the remarkable cataract of disorder.[17] Some valuable insights were gained, notably by the first attempt at a political explanation, by H.R. Trevor-Roper, who argued

in 1959 (against Hobsbawm) that the mid-century revolts were not a class struggle, but political conflict between a court faction (which benefited from the growing strength of the Renaissance state) and a country faction (which suffered the consequences).[18] This idea was disputed by historians of other countries, who were quick to point out Trevor-Roper's Anglocentricity, and to substitute the peculiar political conflicts of their own countries as the driving force of the crisis. R.Mousnier argued that the crisis resulted from conflict between nobility and crown over taxes, as in France during the Fronde, while J.Polišenský argued that it was an international crisis confronting absolutism and parliamentarism, as in Central Europe during the Thirty Years' War.[19]

These divisions were exploited by the sceptics, who argued that the midseventeenth-century crisis was not unique in early modern Europe, but just another cluster of disorders, no different from similar clusters in previous years. P.Clark, J.H.Elliott, I.Schöffer, and J.Topolski separately pointed out spates of popular revolt and political rebellion at periods other than the 1640s: e.g., in the 1560s and 1590s. The economic decline, also, began at different times in different parts of Europe: in northern Italy in the 1580s, Germany and Austria in the 1620s, France and the Baltic in the 1650s, and Castile in the late seventeenth century. England was only slightly affected, and the Netherlands hardly at all. Topolski argued that the seventeenth century saw not a general economic crisis, but 'consolidation' and the beginning of regional differentiation. Others contended that the evidence is flawed, and too regionally various to support the notion of any simultaneous or widespread economic breakdown.[20]

Of most interest in the present context are two theories of the crisis, put forward by R.Brenner and N.Steensgaard, that draw together some or all of the strands in the debate surveyed above. Brenner's 1976 article pulled together the three currents of economic theories of the crisis—the Marxist, the price-cyclical, and the ecological-demographic.[21] Brenner agreed with Romano's emphasis on agriculture, and with Le Roy Ladurie's focus on the relationship between population and resources, but he argued that trade and population cycles did not determine the course of the crisis. Why did the same cycles give rise to different outcomes in different parts of Europe: refeudalization in eastern Europe, peasant farming in France, large commercial estates in England? For Brenner, social structure was the answer: the seventeenth-century crisis was caused by class conflict between lord and peasant, and the differing paths of the crisis were caused by the different relative strengths of these classes in different European societies.

The avenues opened by this analysis were never explored. Historians within national traditions took—sometimes justified—exception to Brenner's delineation of specific national social stmctures.[22] The ensuing debate obscured Brenner's perception that divergent patterns of economic crisis in seventeenth-century Europe may be explained by differences in social structure. The further challenge, of using social structure to explain the divergent patterns of *political*

crisis (revolt and rebellion), was not taken up. Brenner's approach to the economic crisis was not carried over into the political sphere.

Where Brenner had tried to integrate the different economic theories of the crisis, Steensgaard tried to integrate the political with the economic. He argued that the crisis was caused by changes at the interface of politics and economics, changes in distribution, and redistribution. On the one hand, he saw a redistribution *among* states, with growing North Atlantic trade displacing the declining Spanish American and Mediterranean trade. On the other hand, and most centrally, he saw a redistribution *within* states:

> Behind the conflict we find the same thing everywhere, the State's demand for higher revenues… In every case it was the governments that acted in a revolutionary manner: the tax demands disrupted the social balance. They did not create a revolutionary situation: they were in themselves a revolution.[23]

Steensgaard deployed evidence of the huge growth in the resources extracted by the early modern European state, and the military, bureaucratic and fiscal pressure necessary to achieve this. But the avenues opened by this approach (as with Brenner's) were never explored.

III
GERMANY AND THE CRISIS THEORY

All these theories have one thing in common: they leave out Germany. What happens when we put Germany back into the picture?

Germany and the economic theories

Economic theories of the crisis fail the test posed by German territorial fragmentation. Economic cycles varied across German regions, whose economies began to diverge more widely precisely in this period. While commercial agriculture and 'proto-industries' arose in western Germany, east-Elbian Germany refeudalized. Some German regions never recovered from the trade slump of 1619–22 and the hyperinflation of the 1620s, while others emerged relatively unscathed. Parts of Germany (such as Hamburg) flourished through the Thirty Years' War, but most were hard-hit, and still others had begun to decline before the war began.[24] Nor does a cyclical theory account for the German mid-century revolts and rebellions, which were regionally diverse and not closely correlated with economic performance.[25] If the crisis consisted of a cyclical downswing, in Germany it was modified by factors peculiar to each region.

German class structure was also affected by the fragmentation of the Empire and its complex social and institutional structures. Eric Hobsbawm's mid-

seventeenth-century peak in class conflict between the bourgeois and feudal classes cannot be found in Germany. The Imperial institutional framework largely isolated Germany's most powerful bourgeoisie, that of the Free Imperial Cities, from both peasants and nobles. The territorial 'home towns', as Mack Walker shows, obtained princely guarantees of their corporate privileges in this period.[26] The large number of courts, and the immense growth in government in this period, meant that in many German territories, the commercial and industrial bourgeoisie were losing ground to a political bourgeoisie of bureaucrats, *Kameralunternehmer*, court bankers, and others dependent on state favour.[27] Even industrial and commercial bourgeoisies depended increasingly on monopolies, subsidies, and other privileges granted by the prince, often within the framework of corporate or feudal institutions.[28] These adaptive responses, rather than conflict with the nobility, shaped the role of the German bourgeoisie in the crisis.

Their institutional isolation deprived German towns of the incentive to make common cause with peasants against nobles. There was little bourgeois support for early modern German peasant revolts, according to Schultz and Heitz, and little peasant involvement in urban revolts.[29] The German urban conflicts studied by Friedrichs were driven primarily by divisions internal to the bourgeoisie, rather than mirroring feudal-bourgeois class antagonisms.[30]

The German towns' dependence on the state directed conflicts between bourgeoisie and nobility into institutional resolutions rather than violent confrontation. Indeed, the German bourgeoisie supported the military activities of the 'feudal' (aristocratic or monarchical) classes, during the Thirty Years' War, rather than resisting them.[31] Friedrichs finds that there was a slump, not a peak, in German bourgeois revolts between 1620 and 1650.[32] The German crisis did not see the bourgeois victory of Hobsbawm's analysis, at least partly because their symbiosis with princes and nobility limited the growth of German towns.[33] In addition, the Thirty Years' War drained most free and territorial German cities of resources, stifled the industry and commerce on which an independent bourgeoisie would have relied, and strengthened the control of the feudal nobility or the state over 'free' bourgeois institutions.[34] The German bourgeoisie was either weak, or politicized, or unintegrated into territorial society. Its role in the crisis was minimal. Whatever the case with other European societies, in Germany the seventeenth-century crisis was not caused by a bourgeois revolution.

Nor was the crisis caused by Porshnev's or Brenner's peak in feudal conflict between lord and peasant. Feudalism, too, varied across German regions, and was shaped by a wide variety of social and political institutions— in particular, as Heide Wunder argues, by peasant communities and the state.[35] German landholding arrangements were diverse, and in many of them the peasants did *not* confront a feudal aristocracy.[36] In western Germany by the seventeenth century, most peasants had redeemed feudal obligations, or seen them pass to territorial princes, city magistracies, or religious houses. Peasant revolts, even against

'feudal' exactions, were therefore often channelled by the institutional affiliations and privileges of the landlords, rather than by class. In eastern Germany, where 'feudalism' survived, conflict was also channelled by variations in social and political institutions.[37]

Moreover, conflicts *between* peasant and noble classes were often less important than differences *within* the peasant class or *within* the nobility. Early modern German villages were deeply riven, the interests of broad strata below the peasantry opposed to those of the richer 'full peasants', who were often in a minority.[38] These divergent interests prevented unified peasant (or 'rural proletarian') resistance to landlords, or channelled such resistance, where it arose, to the disadvantage of the peasant class as a whole.[39] Nor did the German nobility usually present a united front, even within the bounds of a single territory. Some parts of the aristocracy had become bureaucrats, tax-farmers and lenders to the state. Others derived their income from feudal rents. Still others were unemployed, or in the service of foreign princes, supplanted in military and governmental functions by assorted court Jews and military entrepreneurs,[40] and in bureaucratic office by a new *Staatspatriziat* of bourgeois technocrats.[41] This divergence of economic and social interests generated conflict *within* the aristocracy and reduced cohesion in cases of conflict with other classes.[42] These deep fissures within, rather than between, classes were no less evident at the Imperial level than within particular territories: the Thirty Years' War itself, after all, began as a conflict between coalitions of territorial nobility.[43]

For German peasant revolts, feudal conflict was neither sufficient nor necessary. Indeed, the 'refeudalization' of Brandenburg and Pomerania proceeded in the seventeenth century without serious revolts.[44] Where revolts were most numerous in the period after 1600, in southern Germany and the Habsburg lands, they were directed against taxation or religious regulation as often as against feudal burdens.[45] Even in cases of feudal conflict, the issue was often the intensification of seigneurial burdens by the prince's growing bureaucracy,[46] a pattern particularly common in Germany because the Imperial framework sustained small territories where the prince was the major landlord.[47] Nor can we argue that, when German peasants resisted the state rather than the nobility, they were merely resisting 'centralized feudalism'.[48] There were very few German peasant revolts in which state innovations benefited only the nobility. Indeed, most larger German territories implemented policies of *Bauernschutz* in the seventeenth century, protecting the peasant household and the peasant community against lords for fiscal purposes.[49] Even during 'refeudalization', princes did not always support the economic interests of the nobility; their support for peasant resistance was widespread and deliberate.[50]

Rebellious German peasants, even east of the Elbe, found it worthwhile to invest large sums in lobbying and litigation against their feudal lords in the courts of the territorial lord, and against the latter in Imperial courts.[51] To conduct these cases, peasants sent permanent deputies to princely and Imperial courts, and established corporate peasant 'syndicates', subsequently the focus for

other forms of collective action.[52] Despite the protests of the noble Estates, princes and emperor were increasingly willing to accept such cases.[53] Laws enabling peasant litigation against lords were promulgated in the Habsburg hereditary lands from the 1530s on, and in the large and medium-sized territories of the Empire in the 1560s (e.g., Saxony in 1559); they were intensified everywhere towards the end of the century.[54] Passages enshrining this right increasingly appeared in legal textbooks.[55] The volume of such litigation increased from the late sixteenth century onwards.[56] The prince's courts were consistently prepared to issue peasants with at least redress of grievances, and often favourable judgements,[57] despite complaints from the whole noble Estate that such protracted 'parliamenting' with the rebellious peasants was 'counter to our forbears' custom and privilege'.[58] In early modern German peasant revolts, Schulze finds, 'almost always, concrete and palpable results can be observed, such as reductions in taxes, restrictions on corvée, general pardons for participants in revolts, indeed even changes of ruler'.[59]

In Germany, the ubiquity of peasant litigation against lords, the willingness of peasants to invest resources in it, the favourable judgements delivered, and the rising chorus of noble complaint against it, cast doubt on the argument that the state might judge against individual nobles but not against the interests of the nobility as a class, and thus that peasant revolts against the German state were simply part of 'feudal' conflict.[60] Although not the objective and benign state of conservative historiography,[61] the German *Kleinstaat* was not the 'captured' state, either, a mere tool of the feudal nobility.[62]

Finally, the severe conflicts between nobility and princes in German territories cast doubt on a view of the crisis in terms of feudal conflict between lord and peasant. The Bohemian Revolt in 1618, the Lower Austrian and Moravian revolts of the same date, and the Upper Austrian rebellion of 1626 were all noble rebellions against Habsburg centralization.[63] In 1620, the Saxon nobility resisted their Elector's aid to the Emperor in suppressing the rebellious Bohemian nobles, 'their dear neighbours, their friends through blood and other ties, and also their co-religionists'.[64] In 1625 the Hessian nobility, rebelling against the military, fiscal and religious policies of their Landgrave, put itself under the protection of the Emperor.[65] The nobles of German territories resisted the state because it taxed them, because it forced a state religion on them, because it replaced them in administration and military endeavour by new university-educated technocrats *(Kameralisten)*, and because it competed directly with them for the extortion of peasant surpluses.[66]

The relationships of economic classes with the state and with one another in seventeenth-century German societies covered a spectrum. At one end, there were small principalities where landlord and territorial lord were one and the same, although even here the state cannot be viewed solely as a tool of class conflict between lord and peasant. But, on the other end, there were German territories where a variety of classes, sub-classes, corporate groups, institutions, and individuals (including the state's own bureaucracy and military) all

competed for favours from the state. In addition, the Empire made all German states subject to various levels of pressure and legitimate interference from outside. The characteristics of German popular revolts and noble rebellions suggest that in most German territories by the seventeenth century, the state was the tool of no single class, but rather purchased support (via redistributive and regulatory policies) from many social groups, trading off to the point where the ratio between favours given and support received was roughly equal for each group.[67] This suggests that German absolutism cannot be viewed as merely the centralization of feudalism, and the crisis to which it contributed cannot be simplified as a form of feudal conflict.

Germany and the political theories

But if Germany does not confirm the economic theories of the crisis, it provides even less support for the political theories. For German politics, Trevor-Roper's idea of a crisis between luxurious 'court' and puritan 'country' is all but irrelevant. There were literally hundreds of courts in the Empire, and most were geographically and socially close to the rest of their territories. The pressures for confessional uniformity arising from the Augsburg religious settlement caused German puritan movements either to be outlawed altogether or, as with Württemberg pietism after 1645, to become almost the state religion.[68]

But French high politics come no closer than English to providing a European paradigm. Mousnier views the crisis as a struggle between crown and nobles over taxation, resolved by the emergence of absolutism. His argument that the absolutist state 'resolved' the crisis appears peculiarly inappropriate for German societies, repeatedly destabilized by absolutist taxation and warfare.[69] Moreover, relations between princes and nobles were channelled by factors specific to each European state and, in Germany, by Imperial institutions. The French nobility may have invariably opposed princely centralization and taxation, but in Germany matters were not so simple.[70] Many German nobles were Free Imperial Knights, excluded from all but Imperial and Circle taxes.[71] Other nobles, in the Habsburg hereditary lands for instance, administered and benefited from taxes paid by other classes; despite their own opposition to absolutism, they also opposed popular revolts. The nobles of small southwestern principalities (where feudal and territorial lordship coincided) strongly supported rising taxes, as also in territories such as Mecklenburg, with strong noble estates and a weak bourgeoisie.[72] Elsewhere, as in Brandenburg, the nobility was split, with the larger nobles sharing fiscal responsibility with the prince, and supporting higher taxation, while the Junkers resisted such competition for peasant surpluses.[73] Only the nobles of Saxony, Hessen-Kassel, and the Rhine territories resembled French nobles in resisting princely taxation.[74] Every German society had different fissures between nobility and prince and within the nobility itself. Indeed, political theories of the crisis tend to fail the test of territorial variation for Europe more widely

But there is another reason these theories based on emerging nation-states such as England and France cannot be generalized. Europe still contained a number of 'composite states' (in Koenigsberger's phrase), of which the Spanish Empire and the Holy Roman Empire of the German Nation were only the most outstanding examples.[75] In composite states, politics were channelled by a complex institutional framework of interlocking and competing levels of sovereignty. Indeed, the institutions of the Empire, it will be argued presently, were what gave the German crisis its peculiar form.

Yet even Polišenký's theory of the crisis in terms of Central European international politics fails to take account of Imperial institutions. He describes it as a 'political crisis of the old ruling classes' (principally the Thirty Years' War), driven by a confrontation between a 'Spanish' (absolutist) and a 'Dutch' (parliamentary) conception of society.[76] But this thesis comes unstuck in the very places where explanation is most urgently required. Wallenstein was central to the Imperial (absolutist) war effort, yet was consistently opposed and ultimately murdered by the Spanish faction at the Imperial court. The changing alignment of Electoral Saxony, a key factor in the conflict, was informed by an almost equal suspicion of the threats posed by Catholic absolutism and Calvinist conspiracy to the Imperial constitution.[77] Bavaria, a mainstay of the Imperialist cause, far from being inspired by Spanish absolutism, was primarily motivated by Electoral ambitions internal to the Empire and by suspicion of Spain, which ultimately led it to ally with France. Central Europe's 'international crisis' cannot be explained by a clash of cultures: the constraints imposed by German Imperial institutions must be taken into account.

Germany and the sceptics

No theory of the crisis seems to work for Germany. But neither does the sceptical view. Germany shows unmistakable signs of a peak in social conflict in the first half of the seventeenth century. German societies were racked not only by a civil war which surpassed any other in Europe, but also by the ubiquitous peak in popular protest.[78] Half the peasants' revolts in Brandenburg between 1548 and 1620 took place in the two decades after 1600.[79] Peasant litigation and revolt in Vorderösterreich intensified after the turn of the century.[80] One compendium of supra-local German revolts shows four to six for each quarter-century after 1525, but thirteen for 1625–50.[81] Revolts in small territories show an abrupt decline after mid-century, from nine revolts in the 1650s, to one or two each decade for the rest of the century.[82] Schulze's analysis of German peasant revolts concludes that, while they were less violent, less frequent, and geographically more limited than revolts in England and France, they did clearly peak (at least in Upper Germany and the Habsburg lands) in the early seventeenth century.[83] The German economy, too, was clearly undergoing widespread and profound malaise in this period, although it varied across different territories. Even if the crisis only represents an economic *shift* from

some regions to others, it would need to be explained and analysed. Whether or not we call it a 'general crisis', there is enough evidence of economic, social and political breakdown to require an explanation.

IV
A 'CRISIS OF DISTRIBUTION'

Existing explanations fail because they cannot simultaneously account for the ubiquity of the seventeenth-century crisis and its variation across European societies. Steensgaard comes closest to explaining the *ubiquity* of the crisis when he shows its connections with the single common factor among seventeenth-century European societies, the immense growth of the state. Brenner comes closest to explaining its *variation* when he shows how the crisis was channelled by different social structures. Let us see whether we can explain the crisis better if we bring these together. Did the early seventeenth century see developments, particularly in German-speaking central Europe, which accelerated the pressures on both state and society?

Let us begin with Steensgaard's view of the crisis as one of distribution, caused by the growth of taxation and warfare. Taxes certainly rose throughout Europe in the early modern period, and they rose particularly fast in the first half of the seventeenth century.[84] This would not have been possible without a corresponding increase in the size of bureaucracies and, as Parker shows, the size of armies.[85] Germany moved in the same direction, but its route was determined by the interaction of two levels of state growth, the Imperial and the territorial. Taxation and war began to grow on the Imperial level in the sixteenth-century wars against the Turks and then against the Protestants. Imperial activities stimulated territorial states to grow: directly, when Charles V deployed the precocious Spanish state against Protestantism, and the German Protestant princes mobilized in response; indirectly, when between 1519 and 1606 Imperial taxes increased sixfold, creating (as Schulze shows) the precedent and the need for an increase in territorial taxes.[86] But it was the tensions of the early seventeenth century, in particular the Thirty Years' War, that entrenched the institutions and the consensus for fiscal growth on the territorial level, making it irreversible and ubiquitous.[87] In the decades before the war, and at latest by 1640, each and every German state grew immensely, whatever its prior level of development.

The Habsburg possessions developed early. By 1600, both Emperor (as territorial lord) and Estates had developed sophisticated administrative mechanisms to pay for the Turkish wars.[88] This aroused resistance, muted in Styria and the Austrias, more violent in Vorderösterreich, where the economic base and institutional mechanisms for high taxation were lacking.[89] But even the precocious Habsburg fiscal and administrative mechanisms were revolutionized after the Battle of the White Mountain by devices adopted from the practices of wartime, and from military entrepreneurs such as Wallenstein.[90]

Bavaria was another early developer. The future Elector Maximilian wrote to his father in 1598:

> I see very well that among both spiritual and temporal lords only *ragion di stato* is considered, and that he is respected who has much land or much money, and as long as we have none, we will never have any authority among Italians or others, until we put ourselves into a better position in financial matters.[91]

By 1618, Bavarian taxes had doubled. But here, too, it was the war which caused the real expansion, resulting in the bloody tax rebellion of 1633–4.[92] In 1641 a Bavarian jurist wrote, 'the [war] "Contribution" has unfortunately become so common during this continual twenty-year war that the word is no longer Latin but rather to such an extent German that the peasants in the field understand it altogether too Germanly'.[93] By 1660, Bavarian peasants' payments to landlords had quadrupled since 1480, but their taxes to the state had risen by a factor of twenty-two.[94]

In Saxony, the tax increases of the sixteenth century had been more moderate, and more easily assimilated by the country's industrial prosperity.[95] The Thirty Years' War revolutionized the scale of taxation and the institutions for raising it.[96] Despite repeated parliamentary grants of debt relief and new taxes, many taxes were by the 1630s being levied without consent.[97] A new military tax imposed in 1646 became a regular quarterly levy which continued even when the war was over, enabling the establishment of a standing army,[98] and increasing tenfold by 1700.[99]

States without an advanced fiscal structure before 1618 were forced to develop one. In Württemberg, for instance, central and local bureaucracy had to grow rapidly to administer war taxes: by 1644, more was being spent in three months on billeting alone than in 1620 for an entire year's operations.[100] In Hessen-Kassel, taxes increased continually throughout the war despite the resistance of the Estates, and by 1648, a standing army of twenty regiments had been established.[101] Peace brought only a temporary disarmament. By 1676 the army had returned to twenty-three regiments, soon supported by monthly military contributions.[102]

Brandenburg is the most striking example of the effects of the war.[103] Because Brandenburg avoided the sixteenth-century wars, its administrative and fiscal apparatus remained unsophisticated. Unable to avoid the Thirty Years' War, however, the state broke down altogether in the 1620s, and the territory was devastated.[104] The institutions which emerged at the end of the war reflected raw military and economic might: the post-war Brandenburg Recess, which granted state enforcement of serfdom in return for finance sufficient for a standing army, failed to restore economic or institutional powers to the peasantry or the bourgeoisie, and laid the basis for the strongest state in Europe.

The Thirty Years' War can be viewed as an institutional hothouse. During the war, German princes increased their independence from subjects, began to disregard customary privileges, imitated the administrative and fiscal measures of military entrepreneurs and occupying powers,[105] and raised standing armies which could be used both abroad and against their own subjects.[106] In this hothouse, tardy developers such as Württemberg were forced along towards fiscal, bureaucratic, and military centralization. Early developing Austria, Bavaria, and Saxony grew huge bureaucratic and fiscal structures. In Brandenburg traditional institutions withered, clearing the ground for a sturdy and rapid growth of new ones after 1648. Even small principalities participated in the expansion of taxation and state spending, and reaped the social and economic consequences.[107]

In German (as in other European) territories, this growth in taxation (and its expenditure on warfare) put pressure on an economy without large surpluses, often on economic sectors and social groups least able to bear it. It is thus hardly surprising that the period in which most of this growth took place also saw the peak of economic malaise and socio-political conflict—in German states just as in other states that participated in the war—which we call the seventeenth-century crisis.

But this is only part of the story. The growth of the state in early modern Europe affected more than taxation and warfare. The administrative instruments developed for *these* purposes could also regulate activities previously inaccessible to government, and they could offer redistributive services to a wide range of favoured groups and institutions. Resistance to these *new* forms of redistribution, and competition to control them, were central elements in the crisis. It is here that Brenner's theory of social variation comes in, for this process was channelled by the different social institutions of each European society.

The economy was one of the most important arenas for the growing power of the state. In Germany, as elsewhere, the economy had grown rapidly in the sixteenth century, with increasing regional specialization, expanding trade,[108] and the growth of markets.[109] This economic growth created incentives to compete for political control of products, factor inputs, and the institutions governing their allocation. There is evidence to suggest that in Germany, as elsewhere, by the first half of the seventeenth century, this competition was contributing to the socio-political and economic malaise which we call the 'General Crisis'. But the social outcome of this competition, as Brenner pointed out, varied across European societies. In Germany, the competition was often won by *corporate groups*—village communities, small 'home towns', guilds, and merchant companies—which the state licensed to control each of the different economic sectors.[110] Indeed, corporate groups were central to this competition: in 1643, Elector Friedrich Wilhelm of Brandenburg refused to hear a legal suit by a group of peasants against their lords, on the (controversial) grounds that 'We cannot in

any way approve the peasants' independent undertaking, especially since they do not constitute a corporate body.'[111]

Agricultural growth encouraged landlords in Germany, as elsewhere, to seek to use the new powers of the state to increase feudal burdens, extend monopolies, and curtail peasants' rights.[112] In turn, peasants lobbied, litigated, and rebelled, to defend themselves and to gain princely support for their own interests. Both groups secured some of what they sought. We saw earlier that German princes diversified their sources of social support, by deciding legal cases against landlords, remedying peasant grievances, and instituting policies of *Bauernschutz* which strengthened peasant families and villages, if only to preserve peasant production for state, as opposed to noble, extraction. But alongside *Bauernschutz*, German princes (particularly in the east) also enforced feudal regulations.[113]

How the competition for control of agriculture was channelled by German social structures helps explain some characteristics of the crisis specific to Germany. Schultz points out that peasant revolts lasted longer and were more frequently associated with legal cases in Germany than elsewhere in Europe. *Bauernschutz*, by strengthening the corporate peasant village, provided a strong institution around which legal, as well as violent, resistance to landlords (and prince) could be organized over long periods.[114] Second, from the early seventeenth century, German agriculture went into a long decline.[115] Its stagnation may have been exacerbated by the way in which *Bauernschutz* strengthened communal regulation of agriculture and tended to make it the monopoly preserve of richer peasants,[116] while refeudalization increased labour-intensity and discouraged agricultural innovation.[117]

Industries, too, became arenas of struggle in Germany among different corporate groups—communities, guilds, and merchant companies, competing for state monopolies, wage and price controls, subsidies, tariffs, entry restrictions, labour market regulations, and other legislative favours. Guilds survived in Germany and were strengthened by state legislation. The famous German 'proto-industries' early became the monopoly preserve of privileged corporate groups—the Wuppertaler Garnnahrung, the Calwer Zeughandlungskompagnie, the Uracher Leinwandlungskompagnie—which invested a great deal in lobbying, bribes, and loans to the state, and delivered local regulation in exchange for legislative favours.[118]

Although these institutional features of German industry are well known, the literature does not analyse their macro-economic effects (e.g., on the adoption of new technology, and the growth of output and trade). In so far as corporate competition for extra-economic control of industry favoured established producers, limited output, and raised prices (as monopolies seek to do), and in so far as resources were invested in this competition rather than in productive ends, it would have contributed to the economic downturn.

To sum up: the century prior to the crisis saw a (probably not coincidental) confluence between the growth of the economy and that of the state. Economic

growth increased the value of extra-economic control of economic resources. State growth increased the efficiency with which such control could be exercised. Together, these two growth curves gradually altered the balance of power among the relatively stable social groups and social institutions of the late medieval period. The consequent struggle among classes and repositioning of institutions eventually issued in rebellion and civil war; and, in the economy, in rent-seeking, inefficiencies, and deadweight losses during the adjustment period. This process was channelled by social structures—in Germany, primarily by corporate groups—and this gave the crisis its special shape in different societies.

But taxation and economic conflict are still only part of the picture. The state was also expanding and disrupting the social balance in other ways, especially (as recent local studies reveal) in social and religious regulation. It did this by allying with the two institutions capable of regulating society on the local level: the community and the church. Communities could offer local-level implementation of measures desired by church and central state. The church disposed of immense property, political influence, and its own local-level personnel. In return for more effective enforcement of their own interests, community and church were progressively induced or compelled to place these at the disposal of the growing state.

This involved substantial redistribution of material resources and coercive power. Local community and church bureaucracies grew enormously, cooperated with the state, and widened their activities to education, marriage, the family, welfare, and health.[119] The resulting information and control over social, economic and cultural activities made it possible for the state to redistribute power and resources within communities.[120] Direct or indirect control of ecclesiastical funds and properties helped the state financially.[121] Not least, the spiritual influence of the church lent legitimacy to the political, social, economic and ideological agendas of those who could manipulate it.

This process helps to explain why religion was a mobilizing force in many conflicts associated with the seventeenth-century crisis. In Germany, the *ius reformandi* after 1555 (another artefact of the Imperial constitution) had strengthened territorial princes' control over the church.[122] German state churches—Lutheran, Calvinist and Catholic alike[123]—increasingly imposed religious orthodoxy, social discipline, and bureaucratic control on their subjects.[124] Although this process began in the sixteenth century, the debate about the 'success' or 'failure' of the German Reformation[125] shows that it was often not until the seventeenth century that rulers, clergy, and local officials were able to make Reformation ideas effective on the local level.[126] It was then that it generated social conflict and provoked resistance.

Confessional regulation was a factor in many German peasant revolts (and territorial rebellions) of the period.[127] Where this regimentation was carried out in co-operation with local élites, as in Württemberg, Hohenlohe, Saxony, the Rhine Palatinate, and Brandenburg, it did not provoke open revolt, although there was more conflict than is usually recognized behind the façade of the

harmonious peasant community. In other territories, especially those subjected to the Counter-Reformation, it led to severe revolts: in Upper Austria in 1588–92, in Upper and Lower Austria in 1594–6, in the Salzburg countryside in 1601–2, in Upper Swabia in 1605–8, in Upper Austria in 1626, in the Ennstal of Upper Austria in 1627, in the Hausrückviertel of Upper Austria in 1632, and in the Madeland in 1634–6.[128] Open revolt resulted where religious and social regulation were imposed abruptly, from the centre, and by outsiders—as Jesuits and Bavarians imposed it in Upper and Lower Austria, or Jesuits and Austrians in Bohemia, in the 1620s. It also arose where lords and townsmen, as well as peasants, were united against it, as in Upper Austria in 1597[129] or Upper and Lower Austria in 1626.[130] The chain of Bohemian uprisings before and during the Thirty Years' War (in 1609, 1611, 1618, 1627–8, and 1632) can also be traced to an intensification of regulation which was not just fiscal and political, but also confessional and social.[131]

These local-level processes thus had their counterpart on the grand stage of European politics. The Thirty Years' War, like the English Civil War and many of the European popular rebellions of the mid-seventeenth century, was triggered and perpetuated at least partly by religious initiatives on the part of the state. The Bohemian Revolt was sparked by fear that the prince who was about to become Emperor would extend to the Empire the counterreforming innovations he had introduced in his own Archduchy of Styria ten years earlier. The war spread to the rest of the Empire, and indeed the rest of Europe, because the casting vote in the Electoral College could shift the balance of religious power in the Empire. It was perpetuated by the transfer of Frederick V's electoral dignity to Maximilian of Bavaria (i.e., from Protestant to Catholic), and by measures such as the Edict of Restitution which threatened a massive redistribution of material resources. This is not to say that the seventeenth-century crisis was 'caused' by religious conflict, any more than it was 'caused' by military or economic conflict. The debate has resulted, I would suggest, precisely from the fact that the crisis arose from disequilibration by a *variety* of new forms of coercion. The common feature was not the sorts of activities affected by this coercion—economic, political, or religious—but its common *source*, the rapid expansion of the state into new sectors of life, and the consequent disruption of the balance of power among social groups and institutions.

This common source is reflected in the timing of the waves of revolt in Germany, which corresponded to waves of state expansion. Around 1600, counter-reformation religious regulation in the Habsburg lands evoked a great wave of peasant wars. Between 1580 and 1620 high Imperial taxes and the intensification of territorial policy by the small lordships between the Upper Rhine and the Allgau caused a chain of smaller revolts.[132] The Thirty Years' War, with its fiscal and bureaucratic upheavals, saw a trebling of the frequency of revolts.[133] Immediately after the war, war-induced taxes and redistributive measures caused a new wave of popular revolts: in Kempten, in a number of Swabian principalities, and in western and central German territories.[134] These

waves of state expansion *on a variety of different fronts* fed the tide of social conflict which peaked in the second quarter of the seventeenth century.[135]

The variety of grievances behind the mid-century conflicts prohibits any narrow view of either the early modern state or the resulting crisis. The growth in taxation and war is only part of the story. We must also include the state's growing capacity to alter the allocation of power and resources among social groups by regulating economic transactions, religious beliefs, and social relationships. This revolution in state control called forth a backlash by affected groups, and a struggle among other groups for a share of the spoils. Because social and institutional structures varied, the crisis followed different paths in different European societies.

V
THE GERMAN CRISIS AND ITS INTERNATIONAL OUTCOME

But why did it all happen at once? What accounts for the simultaneity of so many conflicts and dislocations? Attention to Germany can help us answer this central question. We have seen how German states and societies fit common European patterns; let us now examine how Germany was different.

One difference we have seen already: the 'corporative' social structure by which the competition for state redistributive measures came to be organized in German societies. Corporate privileges could be defended and widened only through continual appeal to the state. Particularly for peasant villages, this process of corporate appeal frequently involved playing off the territorial and the Imperial states against one another. The second way in which Germany was different was precisely the Imperial political framework.[136] As H.Weber has shown, the three main forces constituting the Reichstag—the Emperor, the Electors, and the territorial princes—were all attempting to expand their powers at each other's expense in the first half of the seventeenth century.[137] Emperor Ferdinand II expanded his control both in the Habsburg lands and, once in possession of Wallenstein's armies, in the Empire at large. The Electors sought to expand their powers against the Emperor by allying with the other German territorial princes, and against the princes by circumventing the Imperial Diet and negotiating directly with the Emperor.[138] And every German territorial prince was equipping himself with the new fiscal, military and administrative instruments of the early modern state, and resisting Imperial and Circle intervention in domestic affairs.[139] Emperor, Electors and territorial princes were also expanding their powers by concluding foreign alliances: the Emperor with Spain, Poland and the Papacy; Bavaria and Trier with France; and, of course, the Palatinate with the 'Calvinist International'. The Imperial framework was thus responsible for the way in which two levels of aggressively growing state—the territorial and the Imperial—interacted within German society.

These two German peculiarities—of social structure (corporatism) and of political framework (the Empire)—may or may not have been causally related. In any case, they combined to give a special severity to the German crisis. And in the end, it was this combination that triggered the cascade of uncontrollable warfare that accelerated the prior growth of European states to the point of a veritable revolution.

The German economic crisis

For one thing, they made the German economic downturn extraordinarily long-lasting and severe.[140] The Imperial framework perpetuated the survival of small sovereign states, but was too weak to break down their economic autarky. By way of response to the seventeenth-century economic downturn, the small German states used their new powers to intensify barriers against each other, even though their size made them especially dependent on inter-territorial trade. This hindered the regional economic integration which reduced the severity of trade and subsistence crises elsewhere in Europe.[141]

Another factor contributing to the German economic downturn was, of course, the violent and protracted civil war, which was a conflict, not just among territorial states, in various alliances with outside powers, but *between* Imperial expansion and territorial state. Armies, famine and pestilence destroyed labour, while warfare itself destroyed capital infrastructure and increased the riskiness of trade, investment, and technological innovation.[142] And the war prompted many German princes to purchase concessions from influential social groups by agreeing to regulate the economy in their interests, fuelling the costly corporative conflict whose effects have already been mentioned.[143]

The German popular revolts

Corporatism and the rivalry between Imperial and territorial state also shaped German revolts and rebellions. Peasant revolts in Germany were less frequent, violent, successful, or geographically extensive than those elsewhere in Europe; but were longer-lasting. They less frequently focused around representative institutions or encompassed all social groups; they more often combined anti-seigneurial with anti-fiscal grievances; and they were almost invariably accompanied by litigation.[144]

Many of these features arose from the fact that Imperial institutions provided legitimate ways in which rebellious subjects could appeal against decisions of their prince, and interested outside powers could intervene in territorial affairs.[145] Rebellious nobles, towns and peasant villages could appeal to the Emperor directly, to the Imperial supreme courts in Speyer, Wetzlar, Prague and Vienna, or—as in the Bohemian Revolt—to the Imperial constitution.[146] Appeals to the Imperial courts frequently led to the appointment of commissions of enquiry, administered and enforced by the Circles (i.e., by powerful neighbouring

territories).[147] Towards 1600, such Imperial intervention in territorial revolts increased.[148] 'During this period there was hardly a revolt which did not have a parallel court case,' writes Winfried Schulze, 'indeed the opportunities for resolving conflicts by litigation largely determined the tactics of the peasants' resistance.'[149]

German revolts were less violent, because this extensive 'juridification' led to a 'tendency toward giving up force in favour of disciplined, long-term representation of interests'.[150] Outright revolts in Germany were less frequent because juridification diffused, normalized and pacified popular resistance.[151] Since litigation provided an alternative to escalation, and defined peasants' grievances in terms of particular legal relationships and corporate privileges, it tended to prevent German revolts from spreading to neighbouring territories.[152] However, litigation and the other forms of outside intervention legitimized by the Imperial framework made German revolts last longer. The extreme case of this, of course, was the Bohemian Revolt which involved so many outside powers (first within, and then beyond, the Empire) that the ensuing conflict took thirty years to resolve.

German revolts seldom encompassed all social groups at once, because it was easier to appeal to an Imperial institution than to subdue conflicts sufficiently to form alliances, across corporate barriers, with other groups in the same society. German revolts seldom focused around representative institutions (such as the declining Peasant Estates)[153] because Imperial institutions promised rebellious subjects a better chance of being heard. And finally, German revolts often combined anti-seigneurial with anti-fiscal grievances because the Imperial framework sustained many small lordships, which used their growing bureaucracies to reinforce feudal burdens.[154]

The war in the Empire also contributed to the special character of German revolts, creating both the strains which evoked them[155] and the resources, institutions, and authority that quelled them.[156] The battlefields of the Thirty Years' War provided the army of 30, 000 with which Emperor Ferdinand suppressed the Lower Austrian revolt in 1626, and the forces used by Maximilian and the Swedish occupation powers to quell the Bavarian tax rebellion in 1633–4. Thus revolts in Germany were less significant or successful than in other European societies.[157] The Imperial framework 'juridified' some of them, and engulfed the rest in conflict on a larger scale.

The German civil war

It was this larger conflict which most distinguished the German crisis, and which spilled over into Europe at large. The Thirty Years' War exceeded all other early modern European conflicts in violence, duration, and the extent to which outside powers became involved. Many of these characteristics can be attributed to the competition between the Imperial and the territorial state, and to the sheer complexity of their interaction and the number of sovereign entities involved—

this is why the peace process took so long to resolve the conflict. In the war, internal and external resistance to the growth of the state coincided. The war began as an internal rebellion—the Bohemian Revolt—by nobles to whom Imperial institutions gave the right to elect their ruler. This rebellion became a civil war because both Emperor Ferdinand and Frederick V of the Palatinate appealed to institutions and legitimacy outside the territorial state of Bohemia, although within the composite state of the Empire. The Imperial constitution was invoked as a justification by all participants: by Emperor Ferdinand, as territorial lord, claiming the right to impose fiscal and confessional regulation; by the rebellious Bohemian nobility, claiming protection against Ferdinand's expansionist ambitions as territorial lord; and by Protestant Electors and German princes, claiming protection against Ferdinand's expansionist aims as Emperor, particularly his claims to interfere within territorial affairs in matters of religion and internal revolt.[158] With the swing vote in the electoral college at stake, no major German state could afford to remain on the sidelines.

Nor could any state with an interest in strengthening or weakening the power of the church or of the Habsburgs—nor could any state in Europe, in short—afford to remain on the sidelines. So the German war became an international war. Spain and the Papacy could not resist the opportunity of turning back the clock on the gains of the Reformation in Central Europe, and obtaining a stranglehold on the United Provinces. France, Denmark, Sweden, and the United Provinces could not afford to watch the Habsburgs and the Papacy build a powerful territorial state in the centre of Europe. Even without the labyrinth of cross-alliances among German and non-German states, therefore, the Thirty Years' War would have drawn in all the powers of Christendom, and some beyond.

But by intervening in such a violent, protracted and expensive conflict, European powers accelerated the expansion of their own state structures and strained their societies beyond endurance. It was the pressure created by the war which brought together so many currents of resistance to the growth of the state all over Europe at the same time. The war can be seen as shaping the crisis in Europe at large, by accelerating and synchronizing the growth of European states.

Changes in relations between states and societies, therefore, were omnipresent in early seventeenth-century Europe, and (as we have seen) explain many features of the crisis. To the changes in taxation and the public sector, emphasized by Steensgaard, must be added changes in 'extra-economic' competition over economic allocation more widely, emphasized by Brenner. Indeed, as we have seen, studies of the growth of the state on the local level suggest that the resources at issue ranged beyond the economic, to control over almost all aspects of behaviour and belief. Resistance to such control, and a struggle among social groups for their share of its spoils, appears to lie behind the popular revolts, the civil wars, and the economic downturn.

This also explains why the crisis was different in each society: the relationship between state and society varied throughout Europe. In Germany, this

relationship was complicated by widespread corporate privilege and by the Imperial structure, which provided two levels of state to which subjects could appeal to defend and extend their privileges. This fuelled an aggressive competition between Imperial and territorial state which exacerbated the German crisis to the point of civil war.

This war helped to bring relations between state and society in the different European countries to a crisis in the second quarter of the seventeenth century. The high stakes and tangled alliances of civil war within the Empire enticed (or compelled) other European states to intervene, expanding their own state structures in order to do so. The result, both in Germany and in Europe at large, was to produce an all but legendary pinnacle of human misery in an era when the state was expanding faster than the resources available to support it, and provoking costly social conflict over its control. The German crisis, which came to a head in the Thirty Years' War, forced all European states to place intolerable pressures on their subjects. The German crisis, in short, exported disorder to the whole of Europe.

NOTES

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1 Most of the major contributions to the debate are collected in T.S.Aston (ed.), *Crisis in Europe 1560–1660: Essays from Past and Present* (London, 1965); and elsewhere in this volume.

2 English-speaking historians of Germany sometimes refer to the crisis: valuable contributions on aspects of the crisis are made by C.R.Friedrichs, 'German town revolts and the seventeenth-century crisis', *Renaissance and Modern Studies*, 26 (1982), and W.W.Hagen, 'Seventeenth-century crisis in Brandenburg: The Thirty Years War, the destabilization of serfdom, and the rise of absolutism'. *American Historical Review* (1989), 302–35. G.Benecke, The Thirty Years War and its place in the General Crisis of the seventeenth century: Review article', *Journal of European Economic History*, 9 (1980) mentions the crisis in the title without dealing with it in the text. Two historians of the German Democratic Republic reviewed the crisis debate at its high point around 1970: G.Schilfert, 'Revolutionen beim Übergang vom Feudalismus zum Kapitalismus', *Zeitschrift für Geschichte*, 1–2 (1969); H.Langer, 'Eine neue "Krise des Feudalismus"? Zur Diskussion um die sogenannte Krise des 17. Jahrhunderts', *Zeitschrift für Geschichtswissenschaft*, 19 (1971). P.Kriedte's *Spätfeudalismus und Handelskapital* (Göttingen, 1980), translated as *Peasants, Landlords and Merchant Capitalists: Europe and the World*

Economy, 1500–1800 (Leamington Spa, 1983), places the economic crisis (although not the debate about it) at centre stage, pp. 79–126.

3 An exception is H.G.Koenigsberger in 'Die Krise des 17. Jahrhunderts', *Zeitschrift für historische Forschung*, 9 (1982), appearing in shortened form as 'The crisis of the 17th century: A farewell' in H.G.Koenigsberger, *Politicians and Virtuosi: Essays in Early Modern History* (London, 1986), which emphasizes the importance of 'composite states' such as the Empire in shaping the crisis.

4 H.G.Koenigsberger, *The Habsburgs and Europe 1516–1660* (Ithaca/London, 1971), p. 219.

5 E.J.Hobsbawm, 'The crisis of the seventeenth century', in Aston, *Crisis*, pp. 5–58.

6 B.Porshnev, *Les soulèvements populaires en France de 1623 à 1648* (Paris, 1963); and 'Das Wesen des Feudalstaats', *Sowjetwissenschaft GA* (1952), 248–77.

7 M.Hroch and J.Petrán, *Das 17. Jahrhundert—Krise der Feudalgesellschaft?* (Hamburg, 1981; Czech orig. Prague, 1976).

8 J.A.Kosminski, 'Das Problem des Klassenkampfes in der Epoche des Feudalismus', *Sowjetwissenschaft GA* (1952), 248–77, especially p. 260 criticizing Porshnev; and A.N.Cistozvonov, 'Die Genesis des Kapitalismus and ihre Widerspiegelung in den regionalen Typen der Bauernbewegungen in Europa im XVI. bis XVIII. Jahrhundert (Problemstellung)', in G.Heitz *et al.* (eds), *Der Bauer im Klassenkampf: Studien zur Geschichte des deutschen Bauernkrieges und der bäuerlichen Klassenkämpfe in Spätfeudalismus* (Berlin/GDR, 1975), pp. 1–26.

9 For instance, Langer, 'Krise', p. 1398.

10 The debate on this question up to the mid-1960s is summarized in M.W. Netschkins, 'Zu den Ergebnissen der Diskussion über das "aufsteigende" und das "absteigende" Stadium des Feudalismus', *Sowjetwissenschaft: Gesellschaftswissenschaftliche Beiträge*, II (1964); see also Hroch and Petrán, *Das 17. Jahrhundert*, pp. 57–60.

11 P.Chaunu, *Séville et l'Atlantique (1504–1650)* (Paris, 1955–9), VII, 39.

12 R.Romano, Chapter 7, in this volume.

13 J.A.Eddy, Chapter 11, in this volume; G.Utterstroem, 'Climatic fluctuations and population problems in early modern history', *Scandinavian Economic History Review*, 3 (1955), especially 39–44.

14 E.Le Roy Ladurie, *Les paysans de Languedoc* (Paris, 1966), especially I, pp. 8, 652–4.

15 J.A.Faber, 'The decline of the Baltic grain trade in the second half of the seventeenth century', *Acta Historiae Neerlandica*, (1966) 1, has subsequently argued that the demographic downswing, as manifested in the decline in grain shipments through the Oeresund in the second half of the seventeenth century, explains the crisis (p. 130).

16 Seventeenth-century writers aware of the contemporaneous revolts are quoted extensively at pp. 1–2 above; and in Koenigsberger, 'Crisis', pp. 149–59.

17 The two earliest discussions of the crisis (which did not, however, advance explanations for it) were R.B.Merriman, *Six Contemporaneous Revolutions* (Glasgow, 1937); and R.Mousnier, *Les XVIe et XVIIe siècles: Le progrès de la civilisation européenne et le déclin de l'Orient (1492–1715)* (2 vols., Paris, 1954), II, *Le XVIIe siècle (1598–1715)*.

18 H.R.Trevor-Roper, The general crisis of the seventeenth century', in Aston, *Crisis*, pp. 59–96

19 R.Mousnier, *Fureurs paysannes* (Paris, 1967), and 'Research into the popular uprisings in France before the Fronde', in P.J.Coveney (ed.), *France in Crisis, 1620–1675* (London/Basingstoke, 1977); J.V.Polišenský, The Thirty Years War and the crises and revolutions of seventeenth-century Europe', *Past and Present*, 39 (1968); 'Zur Problematik des Dreißigjährigen Krieges und der Wallensteinfrage', *Aus 500 Jahren deutsch-tschechoslowakischer Geschichte* (Berlin, 1958), pp. 106ff, 116; and The Thirty Years War', *Past and Present*, 6 (1954).

20 P.Clark (ed.), *The European Crisis of the 1590s: Essays in Comparative History* (London, 1985); J.Topolski, *Narodziny kapitalizmu w Europie XIV-XVII wieku* (Warsaw, 1965), quoted extensively in Langer, 'Krise', e.g., p. 1413; see Chapters 4 and 5 in this volume; and 'Yet another crisis?', in Clark, *Crisis*, p. 301–3.

21 R.Brenner, 'Agrarian class structure and economic development in pre-industrial Europe', *Past and Present*, 70 (1976).

22 See the essays in T.H.Aston and C.H.E.Philpin (eds), *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-industrial Europe* (Cambridge, 1985).

23 N.Steensgaard, Chapter 2 (quote at pp. 46–7). in this volume.

24 Indeed, German regional diversity has given rise to a long controversy about whether German economic decline was caused by the Thirty Years' War or predated it: see T.K.Rabb, The effects of the Thirty Years' War on the German economy', *Journal of Modern History*, 34 (1962); and H.Kamen, The economic and social consequences of the Thirty Years' War', *Journal of Modern History*, 39 (1968).

25 The Habsburg lands, the Black Forest, the Upper Rhine, and Upper Swabia experienced frequent serious revolts from the late sixteenth century on, while in the north and east there were hardly any, and central Germany was relatively peaceful until the first half of the seventeenth century (see Langer, 'Krise', p. 1398); this timing does not reflect the performance of the regional economies.

26 M.Walker, *German Home Towns: Community, State, and General Estate 1648–1871* (Ithaca/London, 1971).

27 A.Hoffman, 'Zur Typologie der Bauernaufstände in Oberösterreich', in W. Schulze (ed.), *Europäische Bauernrevolten der frühen Neuzeit* (Frankfurt, 1982), pp. 309–22; on this, p. 317.

28 Walker, *German Home Towns;* S.C.Ogilvie, *State Corporatism and Proto-industry: the Württemberg Black Forest 1580–1797* (Cambridge, 1997); G.Heitz, 'Der Zusammenhang zwischen den Bauernbewegungen und der Entwicklung des Absolutismus in Mitteleuropa', in Schulze, *Europäische Bauernrevolten*, pp. 171–90 (on this, p. 173).

29 H.Schultz, 'Bäuerlicher Klassenkampf und "zweite Leibeigenschaft". Einige Probleme des Kampfes in der Zeit zwischen frühbürgerlicher Revolution und Dreissigjährigem Krieg', in Heitz *et al.*, *Bauer im Klassenkampf*, pp. 401–4; G. Heitz, 'Bäuerliche Klassenkämpfe im Spätfeudalismus', in G.Brendler and A. Laube (eds), *Der deutsche Bauernkrieg 1524/25* (Berlin, 1977), p. 207.

30 Friedrichs, 'Revolts'; and C.R.Friedrichs, 'Urban conflicts and the Imperial constitution in seventeenth-century Germany', *Journal of Modern History*, 58 suppl. (1986), S98-S123.

31 W.Reinhard, Theorie und Empirie bei der Erforschung Frühneuzeitlicher Volksaufstände', in H.Fenske, W.Reinhard and E.Schulin (eds), *Historia Integra: Festschrift für Erich Hassinger zum 70. Geburtstag* (Berlin, 1977), p. 174.

32 Friedrichs, 'Revolts'; C.R.Friedrichs, 'Urban conflicts', *passim*.
33 J.de Vries, *European Urbanization, 1500–1800* (Cambridge, 1984), p. 153.
34 W.Rausch (ed.), *Die Städte Mitteleuropas im 17. und 18. Jahrhundert* (Linz, 1981); K.Gerteis, *Die deutschen Städte in der frühen Neuzeit. Zur Vorgeschichte der 'bürgerlichen Welt'* (Darmstadt, 1986), pp. 75–81.
35 H.Wunder, 'Peasant organization and class conflict in eastern and western Germany', in Aston and Philpin, *Brenner Debate*, pp. 91–100.
36 W.Conze, 'The effects of nineteenth-century liberal agrarian reforms on social structure in Central Europe', in F.Crouzet *et al.* (eds), *Essays in European Economic History* (London, 1969), pp. 56–7.
37 H.Harnisch, 'Die Gutsherrschaft: Forschungsgeschichte, Entwicklungszusammenhänge und Strukturelemente', *Jahrbuch für Geschichte des Feudalismus*, 9 (1985), especially 231–4, where he argues that the legal characteristics of peasant leases were probably the most important factor distinguishing areas of *Gutsherrschaft* from those of greater personal liberty for peasants.
38 W.Schulze, 'Europäische und deutsche Bauernrevolten der frühen Neuzeit—Probleme der vergleichenden Betrachtung', in Schulze, *Europäische Bauernrevolten*, pp. 40–1; G.Franz, *Geschichte des deutschen Bauernstandes vom frühen Mittelalter bis zum 19. Jahrhundert* (Stuttgart, 1976), pp. 214ff; J.Lesczynski, 'Forschungsergebnisse der polnischen Historiographie auf dem Gebiete des Klassenkampfes der schlesischen Bauern im Spätfeudalismus', in Heitz *et al.*, *Bauer im Klassenkampf*, p. 483.
39 T.Robisheaux, 'Peasants and pastors: rural youth control and the Reformation in Hohenlohe, 1540–1680', *Social History*, 6 (1981), 281–300; T.Robisheaux, *Rural Society and the Search for Order in Early Modern Germany* (Cambridge, 1989); H.Rebel, *Peasant Classes: The Bureaucratization of Property and Family Relations under Early Habsburg Absolutism 1511–1636* (Princeton, NJ, 1983); W. Schulze, *Bäuerlicheŕ Widerstand und feudale Herrschaft in der frühen Neuzeit* (Stuttgart, 1980), pp. 117–18; T. Barnett-Robisheaux, 'Peasant revolts in Germany and Central Europe after the Peasants' War: Comments on the literature'. *Central European History*, 17 (1984), 384–403 (here 394–9); Hoffmann, 'Typologie', p. 316; Heitz, 'Zusammenhang', pp. 175–7; Lesczynski, 'Forschungsergebnisse'.
40 These were close to 1,500 in number, according to the estimate of R.Redlich, *The German Military Enterpriser and his Workforce* (2 vols., Wiesbaden, 1964–5), I, 170. Polišenský, 'Crises and revolutions', refers to them as a 'new nobility' (p. 34).
41 P.Goubert and J.Meyer, 'Les problèmes de la noblesse au XVIIe siècle', *Thirteenth International Historical Congress* (Moscow, 1970), pp. 7, 9, 11; on this phenomenon within the Austrian *Hofkanzlei*, E.N.Williams, *The Ancien Regime in Europe* (New York, 1970), pp. 369–70; for a detailed territorial study, with material on the seventeenth century, J.Lampe, *Aristokratie, Hofadel und Staatspatriziat in Kurhannover 1714–1760* (2 vols., Hannover, 1963).
42 Hagen, 'Crisis'; generalized by R.Villari, 'Rivolte e coscienza rivoluzionaria nel secolo XVII', *Studi storici*, 12 (1971), 235–64, parts of whose argument are summarized in Koenigsberger, 'Crisis', p. 156.
43 Langer, 'Krise', remarks that 'there was an increase in conflicts between individual factions of the feudal class for the growth in the traditional feudal rent, and

increasingly for the income from trade and commerce as well as from production' (p. 1412).

44 On Brandenburg, see Hagen, 'Crisis'; on how the suppression of peasants in Pomerania took place 'without great class struggle', see B.Wachowiak, *Gospodarcze położenie chłopów w domenach Księstwa Szczecińskiego w XVII w pierwszej połowie XVII wieku* (Szczećin, 1967). p. 353, cited in Langer, 'Krise', p. 1412.

45 Reinhard, 'Theorie', p. 174; P.Bierbrauer, 'Bäuerliche Revolten im Alten Reich. Ein Forschungsbericht', in P.Blickle *et al*. (eds), *Aufruhr und Empörung? Studien zum bäuerlichen Widerstand im Alten Reich* (Munich, 1980), pp. 52–5.

46 Hoffmann, 'Typologie', pp. 309–13; Hagen, 'Crisis', especially pp. 308–10; W. Schulze, 'Peasant resistance in sixteenth- and seventeenth-century Germany in a European context', in K.von Greyerz (ed.), *Religion, Politics and Social Protest: Three Studies on Early Modern Germany* (London, 1984), pp. 82–5. On how antiseigneurial grievances often developed out of anti-fiscal grievances in France as well, see H.Neveux, 'Die ideologische Dimension der französischen Bauernaufstände im 17. Jahrhundert', *Historische Zeitschrift*, 238 (1984), especially 277–8.

47 This is remarked on by both Barnett-Robisheaux, 'Revolts', pp. 391–2; and T.Scott, 'Peasant revolts in early modern Germany', *Historical Journal*, 28 (1985), 455–68.

48 Despite problems with the evidence, this view of the state is applied to early modern Germany by Heitz, 'Zusammenhang', e.g. p. 171: 'The absolute monarchy is the dictatorship of the class of the nobility, more exactly, the dictatorship in the interest of this class'; it is disputed by R.G Asch, 'Estates and princes after 1648: The consequences of the Thirty Years War', *German History*, 6 (1988), 113–32, here especially 121–2.

49 F.Lütge, *Geschichte der deutschen Agrarverfassung von der frühen Neuzeit bis zum 19. Jahrhundert* (2nd edn Stuttgart, 1967), pp. 127ff.

50 W.W.Hagen, 'Capitalism and the countryside in early modern Europe: Interpretations, models, debates', *Agricultural History*, 62 (1988), especially pp. 31, 41.

51 Schulze, *Bäuerlicher Widerstand*. On East Elbian areas, see e.g., Hagen, 'Capitalism', p. 41, and Hagen, 'Crisis'. W.Trossbach, in 'Bauernbewegungen in deutschen Kleinterritorien zwischen 1648 und 1789', in W.Schulze (ed.), *Aufstände, Revolten, Prozesse: Beiträge zu bäuerlichen Widerstandsbewegungen im frühneuzeitlichen Europa* (Stuttgart, 1983), p. 250, argues that 'the peasants made these institutions [the Imperial courts] very much their own and transformed them in their own interest'.

52 Ibid. pp. 247–53.

53 W.Schulze, '"Geben Aufruhr und Aufstand Anlass zu neuen heilsamen Gesetzen". Beobachtungen über die Wirkungen bäuerlichen Widerstands in der frühen Neuzeit', in Schulze, *Aufstände*, p. 273.

54 H.Valentinitsch, 'Advokaten, Winkelschreiber und Bauernprokuratoren in Innerösterreich in der frühen Neuzeit', in Schulze, *Aufstände*, pp. 191–2; Schulze, *Bäuerlicher Widerstand*, pp. 77–80.

55 W.Schulze, 'Die veränderte Bedeutung sozialer Konflikte', in Schulze, *Europäische Bauernrevolten*, p. 286.

56 Schulze, *Bäuerlicher Widerstand,* p. 78; Hagen, 'Crisis', p. 308.
57 Schulze, 'Geben Aufruhr', p. 265; Hagen, 'Crisis'; Trossbach, 'Bauernbewegungen', p. 248.
58 This was the formal complaint of the Upper Austrian nobility in the aftermath of the peasant uprising of 1597. The Estates of Vorderösterreich had made similar complaints in 1594. Trossbach, 'Bauernbewegungen', pp. 247–8; Schulze, *Bäuerlicher Widerstand,* p. 96.
59 Schulze, 'Geben Aufruhr', p. 265.
60 As is argued, for example, by Heitz, 'Zusammenhang', pp. 184–6; by Schultz, 'Klassenkampf, p. 397; and by Leszczynski, 'Forschungsergebnisse', p. 483. Their positions are disputed by J.Petrán, in 'Typologie der Bauernbewegungen in Mitteleuropa unter dem Aspekt des Übergangs vom Feudalismus zum Kapitalismus', in Heitz *et al., Bauer im Klassenkampf,* p. 458.
61 For example, O.Brunner, *Neue Wege der Verfassungs- und Sozialgeschichte* (2nd edn, Göttingen, 1968), pp. 199–212.
62 As portrayed, e.g., by Heitz, 'Zusammenhang', pp. 184–6. On the German *Kleinstaat,* see J.van der Zande, *Bürger und Beamter: Johann Georg Schlosser 1739–1799* (Stuttgart, 1986), especially pp. 138–46.
63 As argued by Langer, 'Krise', p. 1410.
64 Quoted in F.L.Carsten, *Princes and Parliaments in Germany from the Fifteenth to the Eighteenth Century* (Oxford, 1959), p. 229.
65 Ibid. pp. 177–8.
66 On Brandenburg see Hagen, 'Crisis'; on German territories in general see Schulze, *Bäuerlicher Widerstand,* pp. 69–73.
67 V.Press, 'Herrschaft, Landschaft und "Gemeiner Mann" in Oberdeutschland vom 15. bis zum frühen 19. Jahrhundert', *Zeitschrift für die Geschichte des Oberrheins,* NF 84 (1975), especially 210. Trossbach, 'Bauernbewegungen', argues that the courts judging peasant litigation against lords 'made it clear to the *Duodezfürsten* [princes of small states] that even the early modern state could not survive without a minimal consensus and a certain recognition by the governed' (p. 248). This is consistent with certain recent developments in the economic theory of interest groups and political processes: G.S.Becker, 'A theory of competition among pressure groups for political influence', *Quarterly Journal of Economics,* 93 (1983), 371–400, and S.Peltzman, 'Toward a general theory of regulation', *Journal of Law and Economics* (1976), 211–40.
68 P.Schattenmann, 'Eigenart und Geschichte des deutschen Frühpietismus mit besonderer Berücksichtigung von Württembergisch-Franken', *Blätter für württembergische Kirchengeschichte,* 40 (1936), 1–32.
69 Mousnier, 'Uprisings', pp. 161, 164; and *Le XVIIe siècle,* pp. 249–56.
70 R.Mousnier, *Les institutions de la France* I, 85–93.
71 The original Württemberg nobility, by refusing to attend meetings of the Estates, became directly subject to the Emperor in the early sixteenth century, and could levy taxes independently. See J.A.Vann, *The Making of a State: Württemberg, 1593–1793* (Ithaca/London, 1984).
72 Scott, 'Revolts'; but for other examples such as Lübeck and Koenigsberg cf. also M.-L Pelus, 'Lübeck au milieu du XVIIe siècle: Conflits politiques et sociaux, conjoncture économique', *Revue d'histoire diplomatique,* 92 (1978); Koenigsberger, 'Krise', p. 165.

73 Hagen, ‘Crisis’.
74 Carsten, *Princes*, pp. 176ff.
75 Koenigsberger, ‘Crisis’.
76 Polišenský, ‘Thirty Years War’, ‘Crises and revolutions’, ‘Problematik’, pp. 106ff, 116.
77 Carsten, *Princes*, p. 226.
78 This is best documented for France: R.Pillorget, *Les mouvements insurrectionnels de Provence entre 1596 et 1715* (Paris, 1975), ennumerates 108 popular movements in Provence for the ‘peaceful’ period 1596–1635, but many more per annum 1635– 48, and no fewer than 66 in the five years of the Fronde, 1648–53 (p. 988); Y.–M. Bercé, *Histoire des croquants. Etude des soulèvements populaires au XVIIe siècle dans le sud-ouest de la France* (2 vols., Geneva, 1974), counts almost five hundred revolts in southwest France in the period 1590–1715, of which 60 per cent occurred in the period from 1635 to 1650 (pp. 680 ff).
79 H.Harnisch, ‘Klassenkämpfe der Bauern in der Mark Brandenburg zwischen frühbürgerlicher Revolution und Dreißigjährigem Krieg’, *Jahrbuch für Regionalgeschichte*, 5 (1975), 171.
80 C.Ulbrich, ‘Der Charakter bäuerlichen Widerstands in vorderösterreichischen Herrschaften’, in Schulze, *Aufstände*, p. 215.
81 Bierbrauer, ‘Bäuerliche Revolten’, p. 52.
82 Trossbach, ‘Bauernbewegungen’, pp. 225–7.
83 Schulze, ‘Europäische und deutsche Bauernrevolten’, p. 27.
84 Steensgaard, ‘Crisis’, pp. 41–50. For England, M.Braddick, ‘State formation in early modern England’, *Social History*, 16 (1991), suggests that between the 1590s and the 1670s the tax revenue of the state increased by a factor of sixteen (p. 2); according to J.Morrill, ‘What was the English Revolution?’, *History Today* (1984), the English crown doubled its real income between 1603 and 1637 (pp. 11–12). According to E.Le Roy Ladurie, *Les paysans de Languedoc* (Paris, 1966), pp. 294, 481–2, direct taxation alone doubled as a share of gross product in Languedoc in the three decades before the Fronde; the figures in R.Bonney, *The King’s Debts: Finance and Politics in France 1589–1661* (Oxford, 1981), suggest that total taxation in France may have more than doubled in the fifty years before 1630, and may have quadrupled from 1630 to 1640. According to F.Braudel, *The Mediterranean and the Mediterranean World in the Age of Philip II* (2 vols., London, 1972), I, the Venetian and Spanish state budgets tripled in the second half of the sixteenth century, while prices rose by less than two-thirds (p. 33).
85 According to G.Parker ‘The “military revolution, 1560–1660”—a myth?’, *Spain and the Netherlands 1559–1659* (London, 1979), in the forty years before 1630, the armies of Spain increased by half again, those of France doubled, those of England and the Dutch Republic more than doubled, and those of Sweden tripled (p. 96); Parker argues that the cost of putting each of these soldiers in the field rose by a factor of five between 1530 and 1630.
86 Schulze, *Bäuerlicher Widerstand*, pp. 68–9; W. Schulze, *Reich und Türkengefahr im späten 16. Jahrhundert* (Munich, 1978), p. 301.
87 Asch, ‘Estates’, p. 129; G.Schormann, *Der Dreißigjährige Krieg* (Göttingen, 1985), pp. 85–111; G.Livet, *La Guerre de Trente Ans* (Paris, 1963), p. 84; A.Loebl, *Der Sieg des Fürstenrechts auch auf dem Gebiet der Finanzen—vor dem 30jährigen*

Krieg (Munich-Leipzig, 1916); and the summary in T.Mayer, *Handbuch der Finanzwissenschaft* (2 vols., Tübingen, 1952), I, pp. 236–72.

88 W.Schulze, *Landesdefension und Staatsbildung. Studien zum Kriegswesen des innerösterreichischen Territorialstaates (1564–1619)* (Graz/Cologne/Vienna, 1973), pp. 112ff. A parallel to this can be seen in the situation in the Netherlands, with its repercussions on the Spanish state, especially Castile.

89 Ulbrich, 'Character', pp. 207, 213, 215.

90 R.J.W.Evans, *The Making of the Habsburg Monarchy, 1550–1700* (Oxford, 1979), pp. 198–200; K.Richter, 'Die böhmischen Länder von 1471–1740', in K.Bosl (ed.), *Handbuch der Geschichte der böhmischen Länder* (Stuttgart, 1966–72), II, pp. 293–313.

91 H.Dollinger, *Studien zur Finanzreform Maximilians I. von Bayern in den Jahren 1598–1618: Ein Beitrag zur Geschichte des Frühabsolutismus* (Göttingen, 1968), p. 10.

92 Dollinger, *Studien;* also Carsten, *Princes*, p. 394.

93 Quoted in Schulze, *Bäuerlicher Widerstand*, p. 69; there is a pun on 'teutschlich', between 'Deutschlich' ('in German') and 'deutlich' ('clearly').

94 Schulze, *Bäuerlicher Widerstand*, p. 69.

95 Carsten, *Princes*, p. 228.

96 Electoral debts rose from 3.3m guilders in 1622, to 7.1m by 1628, to 11.9m by 1657, despite generous grants and taxes: ibid. pp. 229–30.

97 Ibid. p. 232.

98 Ibid. p. 239.

99 Ibid. p. 232.

100 Ibid. p. 53.

101 Ibid. p. 180.

102 Ibid. p. 182.

103 P.–M.Hahn, 'Landesstaat und Ständetum im Kurfürstentum Brandenburg während des 16. und 17. Jahrhunderts', in P.Baumgart (ed.), *Ständetum und Staatsbildung in Brandenburg-Preussen* (Berlin, 1983), pp. 41–79; Hagen, 'Crisis', pp. 314–18.

104 Hagen, 'Crisis', pp. 314–15.

105 F.Redlich, 'Contributions in the Thirty Years' War', *Economic History Review*, 2nd ser. 12 (1959), shows the ways in which the new forms of finance were developed during the war by Spinola, Mansfeld, Tilly and Wallenstein circumvented traditional institutional barriers to tax-raising, and were subsequently adopted by princes.

106 Carsten, *Princes*, pp. 72–3; Asch, 'Estates', pp. 128–32.

107 Trossbach, 'Bauernbewegungen', concludes that the common denominator explaining simultaneous waves of popular rebellion in small territories of different political structure and geographical position was 'taxation by Empire and Imperial Circle, which accompanied the great wars of the seventeenth and eighteenth centuries' (pp. 240–1).

108 P.Kriedte, H.Medick, J.Schlumbohm, *Industrialization before Industrialization: Rural Industry in the Genesis of Capitalism* (Göttingen, 1977), pp. 26ff; J.de Vries, *The Economy of Europe in an Age of Crisis, 1600–1750* (Cambridge, 1976), pp. 30ff.

109 Kriedte, *Peasants*.

110 For studies of corporatism in action in German economies, see Walker, *German Home Towns*, and the literature surveyed in Ogilvie, *State Corporatism*. The extraordinary prominence of the corporation as an organizational pivot in German socio-political and legal structures is apparent from the work of O.von Gierke, *Das deutsche Genoßenschaftsrecht* (4 vols., Berlin, 1868–1913); G.W.F. Hegel, *Grundlinien der Philosophie des Rechts* (Leipzig, 1911), e.g., p. 394; W. Sombart, *Der moderne Kapitalismus* (2nd edn. Munich, 1928); A.W.Carus, 'Christian Thomasius, corporatism and the ethos of the German professional classes in the early Enlightenment' (unpublished PhD dissertation, University of Cambridge, 1981); and A.Black, *Guilds and Civil Society in European Political Thought from the Twelfth Century to the Present* (Ithaca, 1984).

111 Quoted in Hagen, 'Crisis', p. 317.

112 Schulze, *Bäuerlicher Widerstand*, pp. 69–73; for instance, one grievance of the rebellious Upper Austrian peasants in 1626 was their lords' use of legal monopolies to hinder access to the growing markets for agricultural products: Hoffmann, 'Typologie', pp. 314, 318.

113 These policies included the forcible conversion of leaseholds to hereditary tenures, the compulsory imposition of single-heir inheritance, the development of unified legal structures, and the strengthening of corporate village institutions: F. Lütge, *Geschichte der deutschen Agrarverfassung*, pp. 127ff; O.Hoetzsch, 'Der Bauernschutz in den deutschen Territorien vom 16. bis ins 19. Jahrhundert', *Schwäbische Jahrbuch*, 16 (1902), 237–271; E.Patzelt, 'Bauernschutz in Österreich vor 1848', *Mitteilungen des Instituts für österreichische Geschichte*, 58 (1950), 637–55; Hagen, 'Crisis'.

114 Schulze, 'Peasant resistance', Schulze, 'Europäische und deutsche Bauernrevolten'.

115 Conze, 'Agrarian reforms'.

116 Lütge, *Agrarverfassung;* W.Abel, *Geschichte der deutschen Landwirtschaft vom frühen Mittelalter bis zum 19. Jahrhundert* (Stuttgart, 1959); E.Weis, 'Ergebnisse eines Vergleichs der grundherrschaftlichen Strukturen Deutschlands und Frankreichs vom 13. bis zum Ausgang des 18. Jahrhunderts', *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 57 (1970), 1–14; Robisheaux, 'Peasants and pastors'; Robisheaux, *Rural Society*.

117 J.Topolski, 'La reféodalisation dans l'économie des grands domaines en Europe central et orientale (XVIe–XVIIIe ss.)', *Studia historiae oeconomicae*, 6 (1971), 51–63; J.Nichtweiss, 'Zur Frage der zweiten Leibeigenschaft und des sogenannten preussischen Weges der Entwicklung des Kapitalismus in der Landwirtschaft Ostdeutschlands', *Zeitschrift für Geschichtswissenschaft*, I (1953), 687– 717; J.Kuczynski, 'Zum Aufsatz von Johannes Nichtweiss über die zweite Leibeigenschaft', in *Zeitschrift für Geschichtswissenschaft*, 2 (1954), 467–71; Conze, 'Agrarian reforms'.

118 H.Kisch, 'From monopoly to laissez-faire: The early growth of the Wupper Valley textile trades', *Journal of European Economic History*, I (1972), 298–407; W.Troeltsch, *Die Calwer Zeughandlungskompagnie und ihre Arbeiter* (Jena, 1897); H.Medick, '"Freihandel für die Zunft": Ein Kapitel aus der Geschichte der Preiskämpfe im württembergischen Leinengewerbe des 18. Jahrhunderts', in R.Vierhaus (Festschrift), *Mentalitäten und Lebensverhältnisse: Beispiele aus der Sozialgeschichte der Neuzeit* (Göttingen, 1983); J.Schlumbohm, 'Seasonal

fluctuations and social division of labour: Rural linen production in the Osnabrück and Bielefeld regions and the urban woollen industry in the Niederlausitz, ca. 1700—ca. 1850', in M.Berg, P.Hudson and M.Sonenscher (eds), *Manufacture in Town and Country before the Factory* (Cambridge, 1983); Ogilvie, *State corporalism;* S.C.Ogilvie, 'Institutions and economic development in early modern central Europe: proto-industrialization in Württemberg, 1580–1797', *Transactions of the Royal Historical Society,* sixth series, 5 (1995), pp. 221–50.

119 H.Schilling, 'The Reformation and the rise of the early modern state', in J.D. Tracy (ed.), *Luther and the Modern State in Germany* (Kirkville, Miss., 1986), especially pp. 25–6; K.Blaschke, The Reformation and the rise of the territorial state', in ibid., especially pp. 64–5.

120 Rebel, *Peasant Classes;* Robisheaux, 'Peasants and pastors'.

121 Schilling, 'Reformation', p. 26; Blaschke, 'Reformation', p. 64.

122 H.Lehmann, *Das Zeitalter des Absolutismus* (Stuttgart, 1980), pp. 58–61; M. Heckel, *Staat und Kirche nach den Lehren der evangelischen Juristen Deutschlands in der ersten Hälfte des 17. Jahrhunderts* (Munich, 1968), pp. 67, 224ff.

123 For an example of such regulation under Counter-Reformation Catholicism, see J.Bücking, *Frühabsolutismus und Kirchenreform in Tirol (1566–1665): Ein Beitrag zum Ringen zwischen 'Staat' und 'Kircher' in der frühen Neuzeit* (Wiesbaden, 1972), especially pp. 63–98, 126–41, 175–89; for examples under Lutheranism and Calvinism, see Schilling, 'Reformation', p. 25; on the replacement of communal by autoritarian organization in early Calvinism, see H.Schilling, *Konfessionskonflikt und Staatsbildung: Eine Fallstudie über das Verhältnis von religiösem und sozialem Wandel in der Frühneuzeit am Beispiel der Grafschaft Lippe* (Gütersloh, 1981), p. 189; on the forcible regulation of local religious observance by Calvinist rulers, see G.Strauss, *Luther's House of Learning: Indoctrination of the Young in the German Reformation* (Baltimore, 1978).

124 Rebel, *Peasant Classes;* Robisheaux, 'Peasants and pastors'; Blaschke, 'Reformation'; Ogilvie, 'Coming of age'; Strauss, *Luther's House of Learning;* K. Deppermann, 'Pietismus and moderne Staat', in K.Aland (ed.), *Pietismus und moderne Welt* (Witten, 1974); R.A.Dorwart 'Church organization in Brandenburg-Prussia from the Reformation to 1704', *Harvard Theological Review,* 31 (1938), 275–90; D.Sabean, *Power in the Blood: Popular Culture and Village Discourse in Early Modern Germany* (Cambridge, 1984); G.Strauss, 'Success and failure in the German Reformation', *Past and Present,* 67 (1975); B. Vogler, 'Die Entstehung der protestantischen Volksfrömmigkeit in der rheinischen Pfalz zwischen 1555 und 1619', *Archiv für Reformationsgeschichte,* 72 (1981), 158–196; T.M.Safley, *Let No Man Put Asunder: The Control of Marriage in the German Southwest: A Comparative Study 1550–1600* (Kirksville, Mo., 1984); L.Roper, '"Going to church and street": Weddings in Reformation Augsburg', *Past and Present,* CVI (1985), 62–101.

125 See Strauss, 'Success and failure'; and J.M.Kittelson, 'Successes and failures in the German Reformation: The report from Strasbourg', *Archiv für Reformationsgeschichte,* 78 (1983).

126 Strauss, *Luther's House of Learning;* Bücking, *Frühabsolutismus,* especially p. 73; J.D.Tracy, 'With and without the Counter-Reformation: The Catholic church in the Spanish Netherlands and the Dutch Republic, 1570–1650', *Catholic Historical Review,* 71 (1985), especially 554–9.

127 Bierbrauer, ‘Bäuerliche Revolten’, p. 54; according to Schulze, *Bäuerlicher Widerstand*, local ‘irreligiousness, sectarianism, superstition and [religious] resistance’ led to popular revolts against state imposition of religious orthodoxy in the late sixteenth and early seventeenth centuries (p. 126).
128 Bierbrauer, ‘Bäuerliche Revolten’, p. 54 note 3, and Appendix pp. 65–6.
129 Hoffman, ‘Typologie’, p. 313.
130 Rebel, *Peasant Classes*.
131 J.Koci, ‘Die Klasenkämpfe der Untertanen in den böhmischen Ländern während des Dreißigjährigen Krieges’, in G.Heckenast (ed.), *Aus der Geschichte der ostmitteleuropäischen Bauernbewegungen im 16.–17. Jahrhundert* (Budapest, 1977), pp. 341–9.
132 Schulze, *Bäuerlicher Widerstand*, pp. 51–4; Schulze, ‘Europäische und deutsche Bauernrevolten’, p. 30.
133 These were the Upper Austrian peasants’ war of 1626; revolts in the Ennstal in 1627, in Krain in 1631, in the Hausrückviertel in 1632, in Upper Bavaria in 1633–4, in Lower Styria and Krain in 1635, in the Muehlviertel in 1635–6, in the Zillertal in 1645–7; and an attempted rebellion in Upper Austria in 1648 (Bierbrauer, ‘Bäuerliche Revolten’, p. 10 note 3, and p. 52).
134 Trossbach, ‘Bauernbewegungen’, p. 240; Schulze, *Bäuerlicher Widerstand*, p. 55.
135 Harnisch, ‘Klassenkämpfe’, p. 171; Ulbrich, ‘Charakter’, p. 215; Bierbrauer, ‘Bäuerliche Revolten’, p. 52; Trossbach, ‘Bauernbewegungen’, pp. 225–7; Schulze, ‘Europäische und deutsche Bauernrevolten’, p. 27.
136 Koenigsberger, ‘Crisis’, pp. 159, 166.
137 H.Weber, ‘Empereur, Electeurs et Diète de 1500 à 1650’, *Revue d’histoire diplomatique*, 89 (1975), here especially 292–3, 297.
138 Weber, ‘Empereur, Electeurs et Diète’, pp. 292–3.
139 Trossbach, ‘Bauernbewegungen’, p. 234, argues that this was also true of the smaller territories; it is confirmed in Ulbrich, ‘Charakter’.
140 See, for instance, Kriedte, *Peasants;* Rabb, ‘Effects’, Kamen, ‘Consequences’.
141 On the failure of inter-regional economic integration in Germany, see I.Bog, *Der Reichsmerkantilismus* (Stuttgart, 1959); on its success in the Netherlands, see J. de Vries, *The Dutch Rural Economy in the Golden Age, 1500–1700* (New Haven, CT, 1974).
142 Rabb, ‘Effects’; Kamen, ‘Consequences’.
143 Examples can be seen in Walker, *German Home Towns;* Ogilvie, *State Corporatism;* Ogilvie, ‘Institutions’; Kisch, ‘Wupper Valley’; Medick, ‘Freihandel’.
144 Schulze, ‘Veränderte Bedeutung’, p. 279; Barnett-Robisheaux, ‘Peasant revolts’, pp. 392–3
145 Schulze, *Bäuerlicher Widerstand*, pp. 62–6; Schulze, ‘Peasant resistance’. An interesting example of how rebels used competing state powers for their own ends (in the Schoenberg unrests in Saxony between 1647 and 1680/1) is provided in G.Heitz, ‘Agrarstruktur, bäuerlicher Widerstand, Klassenkampf im 17. und 18. Jahrhundert’, in Schulze, *Aufstände*, p. 155.
146 Schulze, *Bäuerlicher Widerstand*, pp. 76–85, 95–111.
147 Friedrichs, ‘Urban conflicts’, p. 101; Schulze, ‘Veränderte Bedeutung’, p. 283. According to Schulze, *Bäuerlicher Widerstand*, p. 106, Imperial Commissions

were active in all the German peasant revolts that have been investigated for the period 1580–1615.

148 Bierbrauer, 'Bäuerlicher Revolten', p. 59.

149 Schulze, 'Peasant resistance', p. 83.

150 Trossbach, 'Bauernbewegungen', p. 254.

151 On the less violent nature of German revolts, see Schulze, 'Europäische und deutsche Bauernrevolten', pp. 42–3, and Trossbach, 'Bauernbewegungen', p. 246.

152 Trossbach, 'Bauernbewegungen', p. 247.

153 Press, 'Herrschaft', pp. 189, 191, 205, 210.

154 See Barnett-Robisheaux, 'Peasant revolts', pp. 391–2.

155 War created fiscal, military and economic strains which begot revolts: see Schulze, *Bäuerlicher Widerstand,* p. 55. The measures states took in the 1650s and early 1660s to repay war debts, reorganize government and administration, and consolidate the advantages gained by wartime fiscal and bureaucratic precedents and the breakdown of traditional institutions evoked a final severe wave of popular revolts: Trossbach, 'Bauernbewegungen', pp. 236–40, counts 9 revolts in small German territories for the 1650s, 1 for the 1660s, 1 for the 1670s, 2 for the 1680s, 2 for the 1690s, etc. (pp. 225–60).

156 Indeed, as Jean Bodin had written in 1576, 'the most certain means of protecting a state from rebellion, revolts and civil war, and of keeping the subjects contented, is to have an outside enemy' (*Six Books of the Republic* (Paris, 1576), book 5); quoted in H.Haan, 'Prosperität und Dreißigjähriger Krieg', *Geschichte und Gesellschaft,* 7 (1981), 106.

157 Trossbach, 'Bauernbewegungen', p. 246.

158 A.Wandruszka, 'Zum "Absolutismus" Ferdinands II', *Mitteilungen des Oberösterreichischen Landesarchivs,* vol. 14, *Beiträge zur neueren Geschichte: Festschrift für Hans Sturmberger zum 70. Geburtstag* (Linz, 1984), pp. 261–8, especially p. 267.

4
DID HOLLAND'S GOLDEN AGE COINCIDE WITH A PERIOD OF CRISIS?*

Ivo Schöffer

It sometimes seems as if the seventeenth century, wedged between the sixteenth and eighteenth centuries, has no features of its own. With Renaissance and Reformation on the one side, Enlightenment and Revolution on the other, for the century in between we are left with but vague terms like 'transition' and 'change'. To trace the titles of history textbooks dealing with the seventeenth century is often amusing. They frequently illuminate only one aspect, occasionally not very typical, of that century. Sometimes a reshuffling of the international cards is considered most significant: 'From Spanish to French Ascendancy'; or emphasis is placed on the rise of monarchical power: 'The Age of Absolutism'. The same incertitude prevails in economic history: the sixteenth century is grouped round the international phenomenon of the price revolution, the eighteenth century is interpreted in the light of the Industrial Revolution; for the seventeenth century, however, we read only fragmentary treatment, country by country, with mercantilism brought forward as the only common international politico-economic trend.

Now it can be objected that such observations merely strengthen the case against typifying any century at all; indeed, it may appear as a positive advantage when a century for once does not bear an indelible stamp. It is perhaps not justifiable to plot historic development in separate centuries. We have to go back as far as the year AD 1000 to find a turn of the century that was actually realized by its contemporaries as a turning point, and the very disappointment or relief at finding that that year brought no change should warn us against being too hasty in handling full centuries as periods. Trends and developments do not in fact take 31 December or 1 January into account, and pass unnoticed from the year 99 to 100. But in spite of these rational objections, the desire to treat a century as an era does not seem to die down. For example, when a German textbook allows the Reformation and CounterReformation to run on till 1660 and from that point lets a period of Absolutism continue till 1789, our traditional feelings rise in protest whether we wish it or not.[1] Has there never been a seventeenth century, we wonder. Did not that age possess any typical features and peculiarities; was there really no place for such a period in the general development of European civilization? It may be traditionalism, against our better judgement, but we just

simply have to give the seventeenth century a place of its own. Our imagination needs it.

This desire to conceive and depict a period in the history of a greater area than a state, region or city has increased of late. Now that national confines are being felt as oppressive and the wider international coherence of modern nations and states is in any case being admitted, this has also its consequences for our understanding of the past. Attempts to grasp the complete history of mankind straightaway as one whole may have been slightly forced. Contacts between the various parts of the world and the different great civilizations were often quite superficial in the past, and because of this we are once again obliged to take Europe as the focal point. But for regions and countries close to each other in culture and geography, attempts to understand a certain period comprehensively make good sense. This, too, can lead to a dangerous myth formation—such as the magnifying of historic figures like Charlemagne or Charles V into 'European' heroes—and a firm stand should be taken against such anachronisms. But to raise historical writing above politiconational frontiers, to examine experiences common to several European countries, is generally to be applauded, although one need not insist upon such an approach to the exclusion of more traditional historical investigations. For this reason, I should like to discuss seriously and appreciatively the efforts made since the last war to give the seventeenth century an image of its own.

I shall certainly not detract from any other effort in this direction if I pay most attention to Roland Mousnier's impressive and enthralling book on *Les XVIe et XVIIe Siècles,* published in the 'Histoire Générale des Civilisations' series, vol. IV in 1954.[2] Mousnier, an expert in seventeenth-century French history, gives us in this book, among other things, a comprehensive study of the development of Europe in 'his' century. Within the narrow limits of 200 pages he succeeds in summarizing and describing in close inter-relationship the political, economic, social and cultural material—seemingly so disparate and so difficult to survey—of more than one century, i.e., from 1590 to 1715. He achieves this by characterizing the seventeenth century as a *century of crisis*. He announces this theme in his preface:

> The seventeenth century was a time of crisis which affected all Mankind … The crisis was permanent with, so to say, violent shifts in intensity The contradictory tendencies had coexisted for a long time, entangled with each other, by turns amalgamating and combating, and there is no easy way of discerning their limits nor the date at which their relationship changed. Not only did these tendencies coexist at the same time throughout Europe, but even in the same social group, even in the same man, they were present and divisive. The state, the social group and the individual were all struggling ceaselessly to restore in their environment and in themselves order and unity.[3]

For Mousnier seventeenth-century Europe is in a phase of economic decline, intensified into a constant crisis by violent price fluctuations and fierce wars. On the social plane the contrasts become acute: nobility and bourgeoisie fly at each other's throats to preserve their own existence; the rising power of the sovereign steers a middle course through this conflict, confusing and extending it. The broad substratum of the population tries in vain to shake off their lot of hunger and misery by revolt and resistance. Internationally Europe is experiencing the decline of the supra-national power of the Pope and of Habsburg, the rise of an expansionist France with all its consequent wars. In matters of culture, Church and science are in a state of uncertainty and confusion, art flares up in the uncontrolled tragic baroque form. The struggle against this general crisis fails: all sober forms of government organization, of systems of thought such as Cartesianism or of soberness of style as in Classicism, of international ideas regarding balance of power and international law were unable to check the crisis. This period therefore also ends in crisis: economic depression, lengthy, expensive wars, new uncertainty in thought and faith.

Since Mousnier's book this crisis in the seventeenth century has become a familiar concept. Apart from Mousnier's gripping, concise exposition, the word 'crisis' itself struck a spark that turned the dullness of the seventeenth century into colour and brightness. All kinds of seemingly unrelated events and trends that made no contact fell into place in a greater whole. The profound tragedy of the seventeenth century suddenly became apparent. The whole baroque style of art, often so difficult for us to understand and appreciate, received from it a new dimension; its dramatic quality and suspense could be understood as fundamental. The idea of crisis therefore found great favour. Historians who from the history-of-culture point of view were familiar with the baroque concept in more general terms than the strictly stylistic-aesthetic definition seized upon the idea. The idea of crisis appealed strongly to English scholars who, by placing the English 1640–60 revolution within the larger context of European developments, sought to free England from insular isolation. Already before the war, in 1938, the American historian Merriman pointed out that there had been 'six contemporaneous revolutions' which could be studied in their inter-relationship: beside the English revolution he mentioned the Fronde in France, the rebellions in Catalonia, Naples and Portugal and Stadtholder William II's attack on Amsterdam. But Merriman's working out of the ascertained contemporaneousness did not extend beyond a land-by-land description of the points of cross-contact for the various parties in two revolutions at a time. These points of contact were mostly on the diplomatic plane or had led sometimes to one being ideologically influenced by another.[4]

But with the concept of a general European crisis that contemporaneousness acquired a new depth: the six revolutions were as many expressions of the general European tensions, and their contemporaneity proved once more that about 1650 the crisis just *had* to come to a head. When looked for, even more contemporaneous disturbances were discovered: a revolt in Sicily in 1647, in the

Ukraine in 1648, not to speak of local peasant revolts in Austria, Poland, Sweden and Switzerland and a palace revolution in Turkey. The economic historian Hobsbawm elaborated still further the idea of a seventeenth-century economic crisis in a circumstantial article in which he used the Marxist theory of social evolution as a basis.[5] Trevor-Roper took up the idea with a theory entirely his own in which he attempted to show that the crisis was one that had been caused by the abnormal rise in the cost of state organization—court, Church, army and bureaucracy—which, too, had raised so much opposition by their waste and corruption that reforms or revolutions were inevitable.[6] Elsewhere in Spain and Italy, and I imagine also in Germany, the crisis theory was gratefully welcomed, offering as it did an explanation of the decay of these very countries in that century.

In the chorus of delight Dutch voices have been practically silent.[7] This is to be understood, for how can this general crisis be made to square with the Dutch Golden Age? A glance around will show others who did not join in the chorus too gratefully: the Belgians, the Swedes, the Portuguese.[8] For their countries, too, enjoyed in this period at least a reasonable measure of prosperity, even to the extent of flourishing. No, it remains difficult to fit in Holland's period of prosperity and the well-being of other countries with the seventeenth-century crisis. Were they, and the Dutch Republic in particular, only exceptions to the rule? Or, a more searching question, is the crisis maybe a not quite satisfactory characterization of the general condition of Europe? In any case, it is perhaps as well that a word of criticism be heard from the Netherlands themselves. It is a temporary dissonance in the cantata but it may in the long run lead to a better performance.

Every term that passes through many hands gets thumbed, declines in value and becomes worn out. Even in Mousnier, the term 'crisis' is fairly vague. He had to do justice to historical reality by finding a counterpart to his crisis: *'la lutte contre la crise'* and only with the aid of this *pair* of concepts did he succeed at last in catching his century. Can the word 'crisis' really encompass a whole century? Is the term not more appropriate when referring to a condition of short duration, or even a decisive moment of great tension? With other historians, in fact, the 'crisis' began to change and shift in meaning owing to a desire to make it cover their point of view. With Mousnier 'crisis' still had the widest meaning. The *Shorter Oxford Dictionary* calls it 'a state of affairs in which a decisive change for better or worse is imminent'. Among Italians and Spaniards 'crisis' must have acquired rather the meaning of a 'temporary decline in prosperity'. Many English historians attach to it the meaning of 'turning point'. Baroque adherents see it as a category of general civilization: 'disturbance', 'tension'. I readily admit that all these meanings lie very close together and are related to each other. But there remain differences in meaning and when applied, the word may confuse and cause discrepancies.[9]

Moreover, could not the 'crisis' be used for any period? If we consider the points brought up by Mousnier in regard to the sixteenth century we again find the term scattered about in his general characterization of the century of

'Renaissance'. For example, there is in the sixteenth century according to Mousnier *'une crise de la scolastique'* and *'une crise du capitalisme'* and, still more serious, even a *'crise de la Renaissance'* itself. In fact, mankind proceeds from crisis to crisis. But it was surely not just this common piece of human wisdom that Mousnier wished to proclaim? Hobsbawm does, as a matter of fact, go a step further. For him the general crisis of the seventeenth century is a part of a long series of crises said to cover the whole period of transition from feudalism to capitalism, actually beginning in the thirteenth century and only ending in the nineteenth.[10] The seventeenth-century crisis is thus a crisis within a crisis, like a Chinese puzzle. Hobsbawm finds in the seventeenth century *the* crisis *par excellence* because, in his opinion, society only then fundamentally changed. In that very century capitalism at last burst through the chrysalis of the old feudal order.

I would think it fruitless to ponder much about the tendency to characterize the seventeenth century crisis as 'the' crisis. Trevor-Roper goes very far in this direction when he belittles the sixteenth-century changes and esteems them negligible compared with what happened in the seventeenth century.

> Speaking generally, we can say that for all the violence of its religious convulsions, the sixteenth century succeeded in absorbing its strains, its thinkers in swallowing their doubts, and at the end of it, kings and philosophers felt satisfied with the best of possible worlds.[11]

Nor could he keep this up long, for he mentions even in his own article that the Dutch Revolt in the sixteenth century already removed in the northern Netherlands what was to be elsewhere the cause of the seventeenth-century crisis, i.e., the top heavy royal court.[12] Perhaps we should award each century its own crisis, and to my mind also its own contemporaneous revolutions. It is indeed surprising that between 1640 and 1660 we can point to no less than eight such revolts and disturbances, and the supposition that there was some sort of inter-relationship must have some base. But let us just see what other important contemporaneous revolutions exist in other times. I for my part found for the years round 1566, which marks the beginning of the Dutch Revolt, no less than six upheavals, and around the rebellion of Bohemia in 1618 yet another eight (see also Chapter 5, pp. 110 and 114–15 in this volume).

But what in my opinion is useful in this discussion of the general crisis of Europe in the seventeenth century, and deserving of further study and continuing discussion, is the question whether in this crisis any permanent strains and problems played a role that was not only typical of that century alone but was intrinsically linked up with the structure of society. That *ancien régime* possessed, on the one hand, a vulnerability, an instability, that is now no longer to be found in Europe, but, on the other hand, also a constancy, a stability that has also been lost to us. Taking these permanent characteristics as our starting point we can perhaps trace what specific influences in the seventeenth century played a

part and to what extent there was then actually any question of crisis or of anything else. We shall then incidentally have to keep referring to the Dutch Republic and its possibly special character in order to obtain an impression of the place of this Republic in the general pattern of developments in Europe.

Meanwhile, one aspect of the discussion about the seventeenth-century crisis I prefer to avoid, even though I feel obliged to offer some explanation. One adherent of the seventeenth-century crisis theory, Robert Mandrou, has given much time and labour to the cultural phenomena, which, in his opinion, are a clear proof of the crisis.[13] The fact that Mandrou turned his special attention to Spanish cultural expressions weakens his article considerably, for there was in Spain an evident depression which need not directly indicate a general European crisis. Be that as it may, Mandrou points to the pathetic and passionate baroque, if you would call it so, in the Spanish folk literature of that century, lamenting the shocking, heart-rending, bitter sufferings of its own civilization in a kind of 'romantic agony'. But even if we should find exactly the same thing in other literatures and in the visual arts, I doubt if this is a proof of a state of crisis. In art all kinds of elements are so intertwined that we nod our heads to every new explanation that throws light on one aspect. Thus, for example, it was once the vogue to see in baroque something triumphantly grand, a defiant *joie de vivre;* now in contrast we discern something violently moving and a pathetic protest. On both occasions we muttered 'yes indeed'. But are these not simply continually recurrent expressions in some form or other of the eternal strains in life and society, always more acutely felt by the artist than by the man in the street? Could we not call the artist an oscillating needle of the seismography of life rather than of a particular period? How individualistically and distinctively artists react to problems of their life and times! We need only contrast the serenity and harmony in Vermeer's or De Hoogh's art with the tensely grave scenes of Rembrandt and Ruysdael to feel this. Of course we can—and must—time and again make every effort to discover in works of art what was going on in a certain era in certain circles, and we must study style, imagination, traditional forms of expression and technical possibilities to get to know in what manner the artist was at once inspired and directed. Nor do I object to the history of art being involved in general historical research—on the contrary, we 'ordinary' historians do this far too little—but I would like to utter a warning against the many snags and pitfalls to be encountered here. How often do we not find reality filtered through style-standards and tradition, diluted by the personality and the special position of the artist? Thus at this stage I should at the most dare to use the cultural expressions of the seventeenth century as illustrative or interpretative material, not as evidence.

Turning to economic, social and political developments in the seventeenth century, we find ourselves on seemingly firmer ground. But even this is a bog of detail and uncertainty. Let us try to make our way through it with the jumping-pole of generalization To understand the socio-economic reality of the seventeenth century we should keep our eye fixed on the permanent, sometimes

latent, structural phenomena Above all, the continuous tension between food production and food distribution, on the one hand, and the population's food requirements, on the other, created a situation where malnutrition was endemic, hunger often epidemic. This tension remained from the time the population had recovered from the disasters of the fourteenth century until far into the eighteenth century. Malnutrition was so general that even the wealthiest suffered from it, in their case due to ignorance and heedlessness. It is probable that only a relatively small part of the poorest population was permanently hungry, perhaps some 10 per cent of the total: especially town proletariat and vagrant beggars. But hunger could spread like wildfire in unfavourable times of crop failure, high prices or transport difficulties. It is possible that some 40 per cent of the population of Europe may have lived on the verge of starvation due to a one-sided distribution of prosperity and a local scarcity of food production while a seemingly very small cause might precipitate it into the abyss of famine.[14] There is no ignoring the fact—we say from the pinnacle of our achievements—that there was a great lack of organization, of skill and imagination, of economic and technical possibilities that could help to relieve or break this permanent tension, ready at any moment to discharge itself. The fact that society in those days was still preponderantly agrarian, however, tended strongly to reduce this latent tension. In most districts more than 70 to 80 per cent of the people were engaged in agriculture. Famines primarily affected the urban population and only secondarily the rural inhabitants, though, if crops failed, these too could suffer great want. In itself this preponderantly agrarian structure had also a socially stabilizing influence: the peasants lived mostly at a distance from each other and had their own traditional bonds with their landowners, while unrest plagued people living packed together in cities, where emotional relationships with the upper classes were much weaker. Here riots were more frequent than in the country, but when rural unrest did explode it was at least equally violent and atrocious.

So, partly as a result of the permanent, latent tensions, in the seventeenth century also, one insurgency, peasant revolt or civil war followed another. We need only think of the terrible peasant risings that broke out all over France between 1623 and 1648, sometimes true *jacqueries*, disrupting the whole of society and displaying in themselves all the elements of crisis.[15] There were violent epidemics, paralysing crop failures, horrific wars. We need only read Simplicissimus Deutsch or look at Callot's etchings *Les misères de la guerre* to get an impression of the ravages caused by the soldatesque, especially during the Thirty Years' War.

But were these phenomena more frequent and more serious than in previous ages? Or were they only the almost inevitable accompaniments of the old structure of society? Now, generally speaking, it is extremely difficult to plot and compare all the factors properly. We know, for instance, too little about the weather—nor shall we ever get to know enough of this to be able to say whether crop failures were then exceptionally frequent or severe.[16] We also happen to know more of the seventeenth century than we do of preceding centuries because

more archive material of a time closer to us has been preserved and because bureaucratic governments began more intensively than before to record administratively and statistically the fortunes of the population. We are well informed about the plague epidemics of the seventeenth century, but it has been rightly remarked that the reason the Great Plague of London in 1665 remained so long in historical memory was that it was the last violent outbreak of such an epidemic in England. If we count the total number of epidemics in that century and compare it with that of the sixteenth century we are immediately struck by their great frequency in the sixteenth century also, and are more and more confronted with the riddle of why exactly in the eighteenth century these epidemics fell off in frequency and violence. The seventeenth century was not a particularly unfavourable exception, while, on the other hand, a turn for the better took place only in the eighteenth century.[17] If we then point to the serious consequences of the Thirty Years' War for great parts of Germany, it is as well to remember that the French religious wars of the sixteenth century were so destructive that a part of the unrest and misery that followed must be attributed to this period of devastation.

It is odd that the Dutch Republic in the seventeenth century enjoyed comparatively great internal tranquillity Perhaps nineteenth-century historians saw this in too rose-coloured a light, while, in that respect, *we* have become more sensitive to the tensions in the Republic also. It would be foolish to ignore the frequently recurring urban excise riots or the acute labour conflicts in the industrial towns of Holland. But even admitting this, it does remain odd that the Republic was so peaceful as compared with other countries. Only towards the end of the century is there question of a real riot, i.e., in 1672, and also in 1696 on the occasion of the Undertakers' Riot in Amsterdam. This is all the more remarkable when we recall that the province of Holland was not preponderantly agrarian, for here more than 50 per cent of the population was urban. Moreover, as is well known, Holland was densely populated and therefore especially vulnerable to any disturbance in food production and distribution. The Republic's relatively favourable position was due largely to the economic prosperity enjoyed by the United Provinces; a prosperity so elastic that even the poorest people did not suffer real famine despite low wages and permanent malnutrition, which, for instance, became at once manifest in the outbreak of scurvy on voyages lasting more than a fortnight. The favourable circumstances of the Republic were also partly due to good diet. Thanks to cattle-breeding and the fishing industry, people got more protein in their food. Finally, food distribution was decidedly better organized than elsewhere owing to the regular import of grain from the Baltic and well-functioning regular inland shipping.[18]

Therefore, when the pattern of unrest in seventeenth-century Europe is examined, no sufficient reason can be found for saying that this century was in an exceptional situation of crisis. Compared with the complete data of other periods we cannot say that people suffered more economic dislocation than in other periods. And from a regional point of view, the Dutch Republic, more than

any other, stands out like a spot of light on the black canvas of permanent tension and misery.

Economic imbalances and a relative inflexibility, an inadequacy of response, made the economy of the *ancien régime* especially fragile. One line of business or one special product could expand economic activity in a district or country so as to make it top-heavy and susceptible therefore to unexpected collapse. Foreign competition, trade or transport disturbances or internal difficulties might suddenly wither this branch of industry. Also, speaking generally, the purchasing power of a great majority of the population was so low that prosperity in some aspect of trade or industry, agriculture or fishery depended entirely on the needs of a small élite, thus intensifying the fragile character of such prosperity When this precarious demand failed, the whole economy was temporarily shaken, since the rapid development of one activity had involved and distorted the whole internal economy. Catalonia's onesided trade-mindedness brought terrible repercussions with the decline of Mediterranean trade at the end of the fourteenth century. Not even by the sixteenth century had the country recovered from the blow.[19] Similarly, all of Bohemia had great difficulty adjusting to the interruption of silver production in 1540.[20] Castile at the end of the sixteenth century lost the advantageous market for agricultural products in America, which was a blow struck at the heart of the Spanish empire, already staggering under other difficulties.[21]

Just as the seventeenth century had its unexpected economic collapses, so it also had its sudden developments. What happened in this century can best be described as demarcation, on the one hand, and, on the other, redistribution within these demarcated lines. The limits of the possibilities of continuing expansion were made perceptible and demonstrated by the collapse—it cannot be called otherwise—of the Spanish economic exploitation of America. The horizon of that strange, remote paradise had seemed in the sixteenth century to be eternally expanding. New estates, new mines, new masses of Indians had made a repeated shifting of the sites of exploration and exploitation possible. But now in the seventeenth century that territory, at least as far as Europe was concerned, had seemingly reached its limits: in Chile the Spanish troops had met with a bloody repulse; the native population (especially in the Caribbean area and Mexico, less so in Peru), literally decimated by disease and physical debility, could no longer be fully exploited; the mines seem to have become exhausted and the Creoles began to look after themselves and became true Americans, bothering little about their shrinking imports of wine, grain and olives from Spain.[22] Only with difficulty could new riches be amassed by the import of 'black ivory' from Africa and the cultivation of sugar and tobacco: English and Portuguese, Danes, French and Dutch would at the end of the seventeenth century in this way find new prosperity. And the same happened in Asia, though in a less obvious manner. Here the Dutch drove out the Portuguese, but the East India Company had great trouble in actually taking over what the Portuguese had given up. Moreover, the available amount of gold and silver needed for the

proper functioning of Asiatic trade also imposed limits on any new English, French or Dutch initiative. Much later, the Dutch East India Company itself was to start the cultivation of plantations which opened new economic prospects for the Asiatic trade. Thus the hopes cherished for a continuing extension and expansion were extinguished: the overseas expansion of the seventeenth century was stopped by the limits of its own possibilities.

Such limitation can also be deduced from data regarding prices, monetary supplies and population. Mousnier prefers to interpret this as depression and crisis. Especially owing to the great progress made in the historical field of price research, we have many price statistics for the seventeenth century. Mousnier did indeed explicitly include them in his treatise and the whole study and discussion of these has now become the centre of interest for French historians. From these price statistics Mousnier conjures up a serious economic depression for the whole of Europe, with price fluctuations much worse than those of the sixteenth century. Whether it is correct to plot the value of precious metal in graphs, as Mousnier does, so that the picture shows a very unfavourable depression, I venture to doubt.[23] But even if we waive this, it seems unmistakable that the general trend of nominal prices, at least in the latter half of the seventeenth century, tended towards a slump.

Mousnier, and many historians with him, therefore share the ideas of the French economist Simiand who speaks of phase A for the sixteenth century, which is seriously retarded in the seventeenth century and finally turns into phase B about 1650, in which a regular slump becomes visible. And I certainly believe that there is actually such a phase B, a certain economic, and especially an agrarian, regression or depression after 1650.[24] But I mention this with every reserve, and I would not like to speak of a general economic crisis on the grounds of a phase B ascertained from price statistics, even if this phase did begin round about 1650. For I should like to issue a few notes of warning here. First, seventeenth-century slumps are too often judged against the background of a really spectacular boom at the end of the sixteenth century. This was not a normal expansion but something exceptional even for those times of violent fluctuation. Second, I doubt whether so much faith and reliance on price statistics themselves is justified. They reflect after all only a restricted aspect of economic activity, and there is a tendency to attach disproportionate value to them because they are actually the only statistic data we possess for many countries over an extensive period in past centuries. To what extent price statistics taken alone can even mislead was shown by Baehrel's investigation into the economic development of the Basse Provence in the seventeenth and eighteenth centuries. He found for his district in the seventeenth century, in spite of a slump in price statistics, a surprising economic growth. He was able, in fact, to make statistics of the proceeds and value of land, and in both there appeared in the course of the seventeenth century to be even a favourable rise, indicating expansion, reclamation and soil improvement, in short an advantageous increase of production.[25] Third, national and regional variations, even in price statistics,

are much greater than some historians are willing to admit. Not only does the so-called descending line start in every country at different periods—for southern Europe about 1620, for western Europe about 1650, for the Dutch Republic about 1660—but, besides this, there are intercyclic waves which deviate strongly from one another and confuse the general trend picture. In times of general slump years of recovery and improvement can still be very important. As we are inclined, speaking generally, to shorten time in our imagination, the internal fluctuations within a great trend seem of little importance. For the contemporary living in a period in which he might, for instance, experience a recovery three times, intermediary fluctuations were, however, of very great significance as he could not see or even suspect the existence of a general trend.

Mousnier also lays great emphasis on the severe fluctuations from year to year, showing sharper rises and falls than in the sixteenth century. This comparison also requires caution: currency value had fallen considerably and fluctuations on the graph show sharper ups and downs. But in any case there *were* many fluctuations and they had, as we have already mentioned, serious consequences and reflected severe drops in prosperity and social order. But over against these sharp fluctuations stands the fact that in general the price *average* remained at the same level. As compared with the price revolution of the sixteenth century, of which there were offshoots right into the first decades of the seventeenth century, the time that followed shows up very favourably. There is a certain global stabilization that must have had favourable influence on wages. Thus, unlike Mousnier, I should prefer to qualify the fall in the import of silver from Spanish America as a stabilizing factor for economy, which had been ravaged by an all-too-extravagant inflation. I admit at once that the circulation of money was here and there probably slowed down, hampering economic growth, but it seems to me wrong to jump to the conclusion that the total supply of currency was reduced to a minimum. Hoarding and export of precious metals, the latter especially to Asia, did indeed drain money from circulation, but there was always a new supply, especially later on in the seventeenth century, e.g., gold from Brazil; and in the meantime copper met the emergency. Now if we consider conditions in France in the seventeenth century and then calculate the supply of gold and silver (in coinage) together with that century's imports, we shall probably find more money in circulation than in the sixteenth century.[26] Finally, I think we meet the same picture in the general situation of the population. I admit we know very little about seventeenth-century population growth, but from existing data I get the impression of a slight rise rather than of a spectacular drop. The great disasters like plague epidemics (for example, the 1694–5 crisis in France) should not be allowed to confuse this general picture. Such disasters—mostly local—probably kept the people below a maximum of overpopulation but were very likely less sweeping as regards the general level than was once thought.[27]

Thrust back from the path of seemingly never-ending expansion and with a stabilizing tendency as regards prices, monetary circulation and population, seventeenth-century Europe looked quite different to its contemporaries from that

of the turbulent, expansive sixteenth century. How very greatly people were aware of this can be gathered from the many economic treatises on trade and navigation published in the seventeenth century, which are often lumped together as expressions of the economic theory of mercantilism. For here we find the results of the demarcation lines in Europe and of their stabilization: the bounds of the economic possibilities were seen so well that people were convinced, and published their conviction that prosperity would be possible *at the expense* only of other countries.[28] Was it not happening under their very noses? At the expense of the Mediterranean world, at the expense of central Europe, the Atlantic powers were rising; at the expense of the SpanishAmerican colonies the English were beginning their exploitation of Jamaica, the Portuguese their full exploitation of Brazil. Was there not also a bitter struggle among the naval powers? The Dutch Republic strove first against Portugal and later against England and France. Instead of economic growth they saw the marking off of economic boundaries: a fixed trough of economic welfare for all the European pigs to feed from.

Certainly an enormous shift took place in seventeenth-century Europe within the lines of demarcation. The whole weight of commerce and industry moved away from the countries round the Mediterranean, especially Italy and Spain, to the Atlantic coast, to western France, England and the Low Countries in particular. At the same time the balance, assisted partly by the destructive Thirty Years' War, shifted from south and central Germany to the North Sea ports of Hamburg and Bremen and from Poland and the Baltic coast to Scandinavia.[29] The most spectacular fact in this shift was of course the complete collapse of Italy as an important industrial and commercial leading country, and of the Spanish Empire, which had actually only a century before risen to great prosperity and power. It is a tragically engrossing tale to tell how the ancient, proud cities of Genoa and Venice, Leghorn or Pisa fell into decay, while the whole perished glory of Spain assumed a pathetic glamour when one saw pot-bellied galleons of the one-time Spanish merchant marine rotting to pieces in the roads of Cadiz after 1630.

But over against this there are other stories. They are all too well known to the Dutch: Spanish silver that found its way to the Amsterdam exchange bank, even without that famous privateer hero Piet Heyn; the proud palace that rose in Amsterdam and—*mirabile dictu*—was not a palace at all but a city hall; the impressive setting up of trading stations round the whole globe. How vague the term 'crisis' is when used here appears exactly in the interpretation that Hobsbawm gives to it in this respect. For him crisis is also a new birth, which he describes with particular reference to England. The American historian Nef had already spoken of a first industrial revolution in England before 1640.[30] Hobsbawm now expatiates on the increasing concentration of capital, refinement of monetary economy, agrarian changes, all of which laid the foundation for the later, so no longer 'only genuine', Industrial Revolution.[31] Portugal, whom one would have expected to share the Spanish decline, progressed towards new prosperity, first that of Brazilian sugar and later on of gold.[32] Sweden, revived by

copper and iron production, transformed itself within one generation into a—for those days—modern industrial state.[33] Finally, the South Netherlands were really not steeped in such misery as was once thought; trade and industry were developing there in a reasonably favourable way.[34] If this shift is to be called a 'crisis', I have no objection, but it is not unrelieved gloom. Within a process of general stabilization there was a shifting of gravity which brought new countries new profits.

It is clear that of all these countries the Dutch Republic had advanced in probably the most surprising way Leaning on its old staff of trade and navigation, Holland (including Zeeland and Friesland) had pushed to the fore. Undoubtedly in this the Republic had profited by the decline of others. I have already made some slight mention of the fierce attacks on the Portuguese in the Asiatic trading area. We can also call to mind the famous voyage of Dutch skippers to Italy with grain from the Baltic, when overpopulated Italy was in the grip of famine. From that year, 1592, onwards, the Dutch Republic began to make a niche for itself in Mediterranean trade from which Italian and Catalan competition seemed to melt away.[35] In the Baltic area the German and Polish landowning nobility became important connections who, without consulting the ports, could ask low prices for the grain which they were able to produce cheaply by means of increased serfdom.[36] Wherever openings appeared, the Hollanders and the Zeelanders rushed in. To a certain degree, we can call the economic prosperity of the Dutch Republic parasitical, but yet it was also profiting by the general economic shift which was busy moving the centre of gravity to our particular corner of Europe.

Certainly the Republic was bound to Europe in all its fibres. The very onesided stress laid on trade and navigation necessitated this. Hence Holland's and Zeeland's great prosperity is, it would seem, explicable only by the fact that it took place—in the initial phase, the sociologists say—during the time of a general European boom. It was the fascinating Ten Years (1588–98) that the historian Fruin with such insight and intuition has pointed to as crucial for the Republic. It is not without irony, for example, that Spain was then enjoying the same prosperity, only for Spain it was the end and for Holland the beginning; just then the most bulky silver fleets in the whole of the impressive history of Spanish navigation were sailing from Havanna to Seville.[37] And we need only look up the Sound Toll tables to be struck by the unusually high numbers of ships passing through the Sound at the same time.[38] Undoubtedly the subsequent years of possible European depression also cannot have failed to affect the Republic. On this point we have not yet sufficient information; it would seem a most fruitful area of investigation, for example, to examine the consequences of European depression for the Republic, especially the English experience of 1621, which was probably connected to the Spanish low point in the same year. In any case Holland's prosperity waned after 1660, the Republic was also enmeshed in the B phase of European economic development. When studying the economic decline of the Dutch Republic in the eighteenth century, it might be advisable to keep in mind that this declining trend had its starting-point in the

second half of the seventeenth century, despite a trade revival in the last decade of that century and the continuing growth of Amsterdam. In some measure Amsterdam's wealth shrouded this decline. The economic suction power of the great city drew, as it were, the strength of the province to itself. The rural districts pined and the smaller cities languished after 1660. To understand these times we should pay more attention to such dwarfs as Enkhuizen or Hoorn in West Friesland, Gouda or Dordrecht in South Holland as well as to the giant Amsterdam. Unlike the experience of England and Portugal, the B phase in the long run became for the Republic, for instance, such a strangulation that it was no longer able to struggle loose. After Catalonia, Bohemia or Castile we also find in the Dutch Republic an example of a decline as striking as it was enigmatic.

While we can speak of a certain stabilization in the economic relationships, in which striking and sometimes dramatic economic shifts occur, it may be worthwhile to see whether a real crisis in the seventeenth century can be found in the social relationships. Trevor-Roper at any rate concentrates wholly on this aspect for his crisis theory, while Mousnier stresses it equally emphatically. The upward-striving bourgeoisie, says Mousnier, ousted the decaying nobility, the nobility resisted and hunger drove the common people towards support of the one class or the other.[39] But here again, we must be extremely cautious. Was the nobility decaying? Was there a strongly upwardstriving bourgeoisie in particular during the seventeenth century? One is perhaps best reminded of a remark made by Michael Karpovich: 'The trouble with the middle class is that it is always rising.'[40] Certainly it seems as if each era had had not only its crisis but also its 'rising middle class'.

What is usually overlooked, however, is the very firm position that the nobility continued to hold throughout many centuries on the very topmost rung of the social ladder. And the difficulties the gentry had in maintaining themselves lay not so much in the supposed pressure exerted upon them by a continuously upward-striving middle class, as in the fact that internal problems of this nobility led to the danger of enfeeblement and crumbling away, while the middle class usually helped the nobility to overcome this danger at the cost of its own strength. The rigid conception as to the way in which the aristocracy should maintain itself had an undermining effect on its exclusive position. The nobility were obliged to live in a manner that cost a disproportionate amount of blood and money Money especially was every nobleman's Achilles' heel. His revenues were always diminishing when he was unable to occupy himself with them, since other achievements were required of him than economic activity and intelligent book-keeping. The estates of the nobility were always being split up, the relations between the nobleman and his peasants underwent changes that were mostly unfavourable to him; price fluctuations were usually to his disadvantage; and there was a tendency to neglect the estates as a source of revenue in favour of the use of hunting grounds. Impoverished gentry who were unable to keep up their rank for economic reasons were continually dropping out, while the remainder, if they did not wish to eat into their capital to the same extent, were obliged either

to alter their way of life or to tolerate the fact of newcomers intruding upon their station by marriage or ennoblement. The newcomers were of course accepted only if they brought not only new blood but new money, and those that were able to do so were the wealthy citizens and patricians.

For the middle classes this continuous drain towards the nobility meant a serious blood-letting of their own: again and again the wealthiest and most powerful were transfused to fortify the aristocracy, and only replenishment from below and above kept the middle classes going. The effect was, as it were, one of communicating vessels between the classes; it helped to maintain the social hierarchy, but never without trouble and difficulty. For, on the one hand, the middle classes, enfeebled by desertion, resented the strengthened privileges of aristocracy; on the other hand, the destitute among the aristocracy became a truly festering sore on the body politic of the *ancien régime*.[41] In the various religious wars of the sixteenth century the noble desperadoes sometimes played a leading role. It is to Trevor-Roper's credit that he has pointed to the phenomenon of a 'falling gentry' which might have been one of the causes of the English revolution in the seventeenth century.[42] The resentment and discontent of the middle-class townsman had a long history of siege and revolt, too.

But in the sixteenth century in particular a new confusing factor began to play a part: the growth of state centralization. The intensifying of traffic and trade, the new technical possibilities of administration and planning, the national disruption of the Church, the economic interweaving of towns and areas within the boundaries of the State are part of the picture of the systematization of authority and the growth of larger political units. In such a growing unity of the State it was one of the highest noblemen, mostly a prince or a king, who symbolically, and effectively, assumed power. But everything was not drawn into the new whirlpool. Old social tensions remained alive for a long time and regional or local resistance died a lingering death. The sovereign had to take sides, sometimes in favour of the resentful middle class or the impoverished gentry; more often, however, the Crown supported the obedient and order-loving among both middle class and nobility. It was not that the rich and well-born loved royal centralization for its own sake, but their love of order was mostly a sensible and deliberate self-interested calculation. Citizens and nobles might seek and find their own good in the new and old institutions of centralizing power. In the court, army, counting-house and Church they saw their chance to preserve, or preferably to enhance, their own power and riches. It was easier for a nobleman, if he perceived the opportunity, to enter the bastion of government; especially at court and in the army, aristocracy was more acceptable to the sovereign from a sense of caste. Titles and tradition might add extra glamour to governmental position. On the other hand, the middle class was often preferred for high office for the very reason that they were powerless and not too distinguished. So in this new bureaucracy, in this medley of parts of the old classes, of conforming citizens and gentry, the old feudal tradition and aristocratic style might still set the tone.

To other parts of the bourgeoisie and gentry, poor or rich, powerless or mighty, conformity to the growing state was not attractive. Remaining loyal to regional and local ties, feeling menaced in their traditions and customs, these groups provided the source for long-tenacious opposition to centralizing forces. Thus the old motifs of unrest and rebellion, bound up with a previous social hierarchy, continued to play across the new melody of resistance to the systematization of authority, especially in the sixteenth century. In the seventeenth century, however, the cacophony of traditional unrest and new resistance began to die down. The nobility, now at last, economically speaking, safely ensconced in the service of the central power, was obliged for the last time to suffer the pain of replenishment from below by the admission of the *noblesse de robe* and began to close its ranks. The old channels became clogged because the nobility no longer underwent a crumbling process and were able economically to maintain themselves. New and different tensions, however, would in the long run be created by the closing of ranks. For while one vessel's contents remained the same the other began to overflow: for the first time in the eighteenth century appeared the danger of a 'rising middleclass'.

So unrest and the long-continued resistance to centralization of high nobility and cities came to an end in the seventeenth century. The resisting bourgeoisie, always very weak, capitulated sooner than the rebellious groups among the nobility. Class-consciousness and tradition were stronger with the gentry. In the course of the sixteenth century most middle-class resistance had already subsided. Examples may be seen in the futile rising of the Comuneros, Ghent's courageous last stand against Charles V, the final surrender of most of the Hanse cities in the same century The siege of La Rochelle in 1629 can count as the symbolic end of medieval civic power. New metropolises arose in the place of independent cities. They were often open cities, centres of trade and industry, of political power and political government with an enormous concentration of inhabitants, growing rapidly beyond their walls. First came Seville and Lisbon and later Paris and London, not forgetting Amsterdam.[43] The gentry's last resistance was of longer duration, causing great strife and bloodshed, even in the seventeenth century. Finally, in the case of the Fronde the struggle seems to shift from the older, traditional aristocratic opposition to a struggle for the best posts within the central power; and about the same time we see an end to noble resistance in England and in most German states.

Once more the Dutch Republic forms an odd variant. As a result of the revolt against Spain here, unlike in other countries, an end was made to all centralization. And the United Provinces were unique in that the city bourgeoisie of Holland and some other provinces, in contrast to the nobility of most other European republics, was able to concentrate most of the power in itself. Understandably there was some aristocratic opposition: a rudiment may be found in the party struggle where provincial gentry sometimes moved against the bourgeoisie of Holland and Zeeland, and in Holland itself one holder of office remained half an hereditary sovereign and half a powerful nobleman—nor was

he willing always to forget his princely position in the subordinate office of stadtholder. But compared with what happened elsewhere, this old-fashioned gentry opposition was of little significance in the Republic. Another remarkable fact in this respect was the continuing independence of the cities which, against the trend of the times, as it were, actually set their stamp on the Republic.

And yet the apparently irrepressible need for state unification had its effects on the Republic, too. Thanks to Holland's leadership a loose federation of states was drawn closer and given more political coherence. Clumsy institutions, often created for other purposes, were adjusted to form a front of provinces and cities united against the outer world and initially for very reasonable co-operation in home affairs—a co-operation that remained satisfactory so long because trade and navigation enjoyed total freedom of action. The fact that something of the real civic independence disappeared in the long run and that Amsterdam, as the metropolis, absorbed much of what other cities had originally possessed of prosperity and therefore power, also pointed to the necessity of government formation and organization. And if a kindred though later process in connection with the general aristocratization is only casually mentioned, this is because I have spoken about it on an earlier occasion and have now said enough.[44] The Dutch patriciate finally became itself an aristocracy with its own privileges and fewer duties, adapting within two or three generations an aristocratic way of life and philosophy The bureaucratic administration of the federation of states became nursery and bulwark for aristocratization.

I have repeatedly spoken of 'stabilization' and sometimes of 'shift' instead of crisis. To a certain extent the seventeenth century was, as I see it, a period of solidifying and organizing. Inside Europe the prices, currency circulation and the growth of population became more settled and stable. Economic expansion overseas came up against inevitable contemporary setbacks and restrictions. And, while at home there are startling economic shifts, there was in the economic sphere a certain retardation, maybe even regression. The state achieved its growth, and society, convulsed and still being convulsed by wars and uprisings, seemed slowly and with difficulty to be settling down. At home the social process for the first time lost part of its own unrest. I do not deny that great suffering was caused by all these shifts and strife, and if that has to be called 'crisis' I have no objection, but it is then rather the permanent crisis of the *ancien régime* that is seen slightly to diminish precisely during the seventeenth century.

Many inhabitants of Europe found themselves suddenly enclosed like mice in a glass cage. This was for that matter not more so than it used to be, and it would soon appear that there were again sufficient loopholes. But while mice so caged can do nothing except vainly scratch the walls, devour each other or copulate, human beings behaved differently. Naturally even among these there were some who slid round the walls with outspread arms, many devoured each other and most of them behaved as people always do in happiness and distress. But there were some who examined their own cage, surveyed their boundaries of will power and ability, drew up laws and regulations of their own imprisonment and

tried to create peace and order in their own society. There were some who occupied themselves with introspection and searchings of the heart. In this process of coming to their senses I would venture to include the Scientific Revolution of the seventeenth century, the growing felt need of understanding microcosms and macrocosms by the aid of Reason; also the new theories regarding the power of the state and shared sovereignty, monarchic power and monarchic absolutism. It is possible that here too lies a basis of explanation for the inner deepening of religious life as found by the Jansenists or the Huguenot fugitives. Probably, too, it was brightened by the gravity and inwardness of baroque as it reveals itself in Dutch painting. For even though our Golden Age does not seem to fit entirely into that period of consolidation, it was consistent with it and shared in it. While here and there attaining to unexpected heights, the Republic was yet every time drawn back to its own place at a junction of waterways, among the great powers, among the civilizations of the West, drawing breath with the rise and fall of the destiny of European nations.

NOTES

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1 See *Propyläen Weltgeschichte*, V (1930) and VI (1931).

2 R.Mousnier, *Les XVIe et XVIIe Siècles. Les Progrès de la Civilisation Européenne et le Déclin de l'Orient (1492–1715), Histoire Générale des Civilisations*, ed. M. Crouzet, IV (Paris, 1954).

3 Ibid., p. 143. Translated by the editors of the present work.

4 R.B.Merriman, *Six Contemporaneous Revolutions* (Oxford, 1938), an elaboration of a David Murray Lecture published under the same title (Glasgow, 1937).

5 E.J.Hobsbawm, 'The crisis of the seventeenth century', first published in *Past and Present* V; VI (1954), now reprinted with a postscript, in Trevor Aston (ed.), *Crisis in Europe 1560–1660. Essays from Past and Present* (London, 1965), pp. 5–59.

6 H.R.Trevor-Roper, 'The general crisis of the 17th century', first published in *Past and Present* (1959), now reprinted in Aston, op. cit., pp. 59–97. See also 'Discussion' of E.H.Kossmann, E.J.Hobsbawm, J.H.Hexter, R.Mousnier, J.H.Elliott, L.Stone with 'Reply' of H.R.Trevor-Roper in *Past and Present*, XVIII (1960), pp. 8–42, only partly reprinted in Aston, op. cit.

7 One exception: B.H.Slicher van Bath, *The Agrarian History of Western Europe A.D. 500–1850* (London, 1963), paid full attention to the crisis (especially pp. 206–21). In agriculture itself, which came for the Republic in the second or third place and was the first to suffer from a depression, a slump was sooner discernible.

8 It should be said, however, in all fairness that on Sweden and Portugal 'crisis' essays have been written: M.Roberts, 'Queen Christina and the general crisis of the

seventeenth century', reprinted in Aston, op. cit., pp. 195–223 and P.Chaunu, 'Brésil et Atlantique au XVIIe siècle', in *Annales E.S.C.*, XVI (1961), pp. 1176– 207.

9 These discrepancies can lead to very great divergences even in the dating of the crisis. French historians see the crisis reach its lowest point only after 1650; English historians, on the other hand, see the end of the crisis already in 1660. Compare for this the title of Aston, op. cit.

10 Hobsbawm, op. cit., p. 5.

11 Trevor-Roper, op. cit., p. 62. Compare also the following sentence on the same page: 'the sixteenth century goes on, a continuous, unitary century and society is much the same at the end of it as at the beginning. Philip II succeeds to Charles V, Granvelle to Granvelle, Queen Elizabeth to Henry VIII, Cecil to Cecil; even in France Henry IV takes up, after a period of disturbance, the mantle of Henry II. Aristocratic, monarchical society is unbroken: it is even confirmed.' This is striking but deceptive rhetoric: between Henry VIII and Elizabeth or Henri II and Henri IV lie worlds of difference. In this way we can just as well paraphrase developments in the seventeenth century: 'Philip IV succeeds to Philip III, Don Luis de Haro to Olivares, Louis XIV to Louis XIII; even in England Charles 11 takes up, after a period of disturbance, the mantle of Charles I.'

12 Cf. E.H.Kossmann's remarks on this subject, in *Past and Present*, XVIII (1960), p. 10.

13 R.Mandrou, 'Le baroque européen: mentalité pathétique et revolution sociale', in *Annales E.S.C.*, XV (1960), pp. 898–914.

14 All percentages given here are of course very crude estimates, mainly based on urban data, substantially reduced for the more favourable situation on the countryside. Regional differences can be very great.

15 Cf. B.F.Porshnev, *Les Soulèvements populaires en France de 1623 à 1648* (Paris, 1963). This work appeared in Russian in 1948.

16 While hot summers or long winters, violent storms or other exceptional weather phenomena have been recorded and can be treated statistically, there is too much regional difference, in particular in respect to irregularity in the degree of humidity and rainfall, to allow for complete statistical data on harvests and crop failure in connection with weather conditions. The yield ratios also confirm this (cf. B.H. Slicher van Bath, *Yield Ratios 810–1820* (Wageningen, 1963)).

17 For the remark on the London epidemic of 1665, see J.Saltmarsh, 'Plague and economic decline in the latter Middle Ages', in *Cambridge Historical Journal*, VII (1941–3), p. 32 ('is remembered not because it was the greatest but because it was the last London plague'). [For two surveys of plague see J.F.D.Shrewsbury, *A History of Bubonic Plague in Britain* (Cambridge, 1970), and J.N.Biraben, *Les Hommes et la peste en France*, 2 vols (Paris, 1975)—eds.]

18 The favourable factors mentioned here are given merely as a working hypothesis, partly due to yet unpublished remarks on this subject by E.Jonxis and B.H. Slicher van Bath. This deserves further elaboration and further studies. [But see the recent list of riots in R.M.Dekker, 'Oproeren in de provincie Holland, 1600– 1750', in *Tijdschrift voor Sociale Geschiedenis*, IX (1977), pp. 299–329—eds.]

19 P.Vilar, *La Catalogne dans l'Espagne Moderne*, I (Paris, 1962).

20 J.U.Nef, 'Mining and metallurgy in medieval civilisation', in *Cambridge Economic History*, II (Cambridge, 1952), pp. 469–70.

21 J.H.Elliott, 'The decline of Spain', in Aston, op. cit., pp. 187–95. See also his *Imperial Spain, 1469–1716* (London, 1963), especially pp. 286–95.
22 P.Chaunu, *Séville et l'Atlantique (1504–1650)*, especially VIII, 2 (2 vols) (Paris, 1959). As this gigantic work is difficult to handle, see also the short résumé P. and H.Chaunu, 'Economie atlantique. Economie mondiale (1504–1640)', in *Journal of World History* (1953–4), pp. 91–104, and here especially p. 97.
23 Cf. R.Baehrel, *Une Croissance: La Basse Provenence Rurale (Fin du XVIe Siècle—1789), Essai d'Economie Historique Statistique* (Paris, 1961), especially pp. 12–20. Cf. also my review, in *Tijdschrift voor Geschiedenis*, LXXVII (1964), pp. 17ff.
24 Slicher van Bath, *Agrarian History*, p. 206, calls this depression one 'of a far milder sort than the serious economic decline of the late Middle Ages'. The depression was especially serious for so-called under-developed areas; while it could work on the other hand in a rationalizing and intensifying direction: spread of cottage industries, specialization in cattle-breeding, wine-growing, market gardening, etc.
25 Baehrel, op. *cit., passim*.
26 Ibid., pp. 33–41 and Planche VI. See also pp. 189–94 below.
27 Regarding the population curve in general and also in the seventeenth century, cf. my 'De demografie van het oude Europa' in *Tijdschrift voor Geschiedenis*, LXXIV (1961), pp. 1–31. As to the possibly slighter consequences of disasters than presumed see Baehrel, op. cit., pp. 231–306, especially pp. 267- 9.
28 This is sharply drawn by D.C.Coleman, 'Eli Heckscher and the idea of mercantilism', in *Scandinavian Economic History Review*, V (1957), pp. 3–25, in particular, pp. 18–20.
29 Cf. articles connecting this economic shift with the developments and influence of Reformation and Counter-Reformation: K.Samuelsson, *Religion and Economic Action. A Critique of Max Weber* (New York, 1961), pp. 102–37; H.R.TrevorRoper, 'Religion, the Reformation and social change', in *Historical Studies*, IV (5th Irish conference of historians) (London, 1963), pp. 18–45 and H.Luethy, 'Once again: Calvinism and Capitalism', in *Encounter*, XXII (1964), pp. 26–39.
30 J.U.Nef, 'The progress of technology and the growth of large-scale industry in Great Britain, 1540–1640', in *Essays in Economic History*, I, ed. E.M.CarusWilson (London, 1954), pp. 88–107. The idea is also referred to in other publications by Nef.
31 Hobsbawm, op. cit., pp. 31–41.
32 F.Mauro, *Le Portugal et l'Atlantique au XVIIe Siècle 1570–1670. Etude Economique* (Paris, 1960). Cf. Chaunu's attempts to incorporate the Portuguese boom after 1650 in the crisis theory: P.Chaunu, 'Brésil', esp. pp. 1192–6.
33 E.F.Heckscher, *An Economic History of Sweden* (Cambridge, Mass., 1954), pp. 79–130.
34 J.A.van Houtte, 'Onze zeventiende eeuw "ongelukseeuw"?' (Brussels, 1953) (*Meded. Kon. VI. Acad. v. Wetensch., Lett. en Sch. Kunsten Kl. der Lett.*, XV, 8).
35 Cf. Z.W.Sneller, 'Het begin van den Noord-Nederlandschen Handel op het Middellandsche-Zeegebied', in *Verslag Prov. Utr. Gen. v. K. en W.*, 1935, pp. 70–93; J.H.Kernkamp, 'Scheepvaart en handelsbetrekkingen met Italie tijdens de opkomst der Republiek', in *Economisch-historische herdrukken* (Den Haag, 1964),

pp. 199–234; and F.Braudel, *La Méditerranée et le Monde Méditerranéen à l'Epoque de Philippe* II (Paris, 1949), pp. 494–503.

36 F.Vollbehr, *Die Holländer und die Deutsche Hanse* (Lübeck, 1930), pp. 20–9.

37 P.Chaunu, *Séville et l'AtIantique*, VII (Paris, 1957), pp. 44–5. It starts about 1580, reaches its summit about 1608. These high tonnages do not, however, reflect fully the actual flourishing of the Spanish-American trade, cf. ibid., VIII, 2, pp. 667–1070.

38 Nina Ellinger Bang (ed.), *Tabeller over Skibsfahrt og Varentransport gennem Oresund, 1497–1660*, I (Skibsfarten) (Kopenhagen-Leipzig, 1906), pp. 86–171. High figures start about 1580 and remain high till after 1620.

39 Mousnier, op. cit., pp. 152–60.

40 Quoted by D.Gerhard, *Alte und Neue Welt in Vergleichender Geschichtsbetrachtung* (Göttingen, 1962), p. 55 (see also *American Historical Review*, 61 (1955–6), p. 912).

41 Here I owe much to J.H.Hexter, *Reappraisals in History* (London, 1961), in particular: 'Factors in modern history' (pp. 26–45) and 'The myth of the middleclass in Tudor England' (pp. 71–117). What I made of it is of course for my account.

42 H.R.Trevor-Roper, *The Gentry 1540–1640* (London, 1953).

43 Kindred ideas to be found with J.P.Cooper, 'Differences between England and continental governments in the early seventeenth century', in *Britain and the Netherlands*, I (London, 1960), pp. 62–91, especially pp. 62–4.

44 Cf. my *Ons Tweede Tijdvak* (Arnhem, 1962).

5
REVOLUTION AND CONTINUITY IN EARLY MODERN EUROPE*

John Elliott

Of all the debates that have agitated historians during the past few years, none has been more lively, or less conclusive, than the great debate surrounding what has come to be known as 'the general crisis of the seventeenth century'. While dissenting voices have been raised here and there,[1] the current fashion is to emphasize the more turbulent characteristics of the age. It was in 1954, which seems in retrospect to have been an unusually crisisconscious year, that Professor Roland Mousnier published a general history of sixteenth- and seventeenth-century Europe, in which the seventeenth century was depicted as a century of crisis, and especially of intellectual crisis.[2] In the same year, Dr Hobsbawm, in an article that now stands as the classic formulation of the 'general crisis' theory, argued that the seventeenth century was characterized by a crisis of the European economy, which marked a decisive shift from feudal towards capitalist organization.[3] Since then, Professor Trevor-Roper, with one eye on the political revolutions of the 1640s and the other on Dr Hobsbawm, has produced a uniquely personal interpretation of the seventeenth century as an age of crisis for the 'Renaissance State'.[4]

It is, I think, striking that three such distinguished historians, of very different views and persuasions, should have united in depicting the seventeenth century in such dramatic terms. They all represent some aspect of the age— whether economic, intellectual or political—in terms of discontinuity, in the sense either of a change of direction or of a change of pace. The change, too, is a violent one, as the use of the words 'crisis' and 'revolution' suggests. But the crisis of one historian is a chimera to another, and the consensus collapses as soon as attempts at definition begin.

It is not my intention now to embark on the daunting task of reconciling the irreconcilable. Nor do I intend to examine the evidence for and against an interpretation of the seventeenth century as an age of economic and intellectual crisis. Instead, I have chosen to concentrate on the narrower, but still, I think, important, question of the 'political' revolutions of the middle years of the century—those revolutions that (in Professor Trevor-Roper's words) 'if we look at them together…have so many common features that they appear almost as a general revolution'.[5]

The revolts and upheavals that may be held to constitute this 'general revolution' have frequently been listed: the Puritan Revolution in England, flanked by the revolts of Scotland and Ireland; the insurrections in the Spanish monarchy—Catalonia and Portugal in 1640, Naples and Palermo in 1647; the Fronde in France between 1648 and 1653; the bloodless revolution of 1650, which displaced the *stadtholderate* in the Netherlands; the revolt of the Ukraine from 1648 to 1654; and a string of peasant risings across the continent. Nor should we disregard the plea of Professor Michael Roberts that 'if we are really determined to bring the Cossacks and the Ironsides within the scope of a single explanation', we should not 'leave Sweden out of the reckoning'.[6] For did not the year 1650 see a dangerous social and constitutional crisis in the troubled realm of Queen Christina?

This clustering of revolts was a subject of fascinated concern to contemporaries, who saw them as part of a great cosmic upheaval; and it has frequently been commented upon by historians. It is thirty years now since Professor R.B.Merriman published his *Six Contemporaneous Revolutions*. But for Merriman the six revolutions afforded 'an admirable example of the infinite variety of history'.[7] Since the 1950s, however, the tendency has been to emphasize their similarities rather than their differences; and the concept of a 'general revolution' of the 1640s has effectively come to influence the history of seventeenth-century Europe only in our own generation.

Not the least of the attractions of a 'crisis' interpretation of the seventeenth century to our own age is that it offers the possibility of a unified conceptual approach to a complex period. It has, too, the additional advantage of plausibility, with that dramatic decade of the 1640s to bear witness to the turbulence of the times.

Opinions may vary about the long-term consequences of the revolutions. Not everyone, for instance, would agree with Professor Trevor-Roper[8] that the seventeenth century 'is broken in the middle, irreparably broken, and at the end of it, after the revolutions, men can hardly recognize the beginning'. But there would probably be a fairly general measure of agreement with his view that 'the universality' of revolution in the seventeenth century pointed to 'serious structural weaknesses' in the European monarchies—weaknesses that gave rise to revolutionary situations.

Whether these weaknesses were more or less serious at this moment than in preceding generations need not at present concern us. All I wish to do for the time being is to draw attention to the way in which the argument is couched. It is the 'universality' of seventeenth-century revolution that points to structural weakness. This argument from universality underlies most of the theories about the 'general crisis' of the seventeenth century Six contemporaneous revolutions (at a minimum count)—does not the very number and pervasiveness of revolutionary movements suggest a moment of unique gravity, and a crisis of unique proportions, in the history of Early Modern Europe?

But supposing this unprecedented epidemic of revolutions was not, after all, unprecedented. Let us look back for a moment to the sixteenth century, and in particular to the decade of the 1560s: 1559–60, revolt in Scotland, culminating in the abdication of Mary Queen of Scots in 1567; 1560, revolt of the Vaudois against Emmanuel Philibert, Duke of Savoy; 1562, outbreak of the French civil wars; 1564, revolt of the Corsicans against Genoa; 1566, the beginning of the revolt of the Netherlands; 1568, revolt of the *moriscos* of Granada; 1569, the Northern Rebellion in England. Seven 'contemporaneous revolutions'; and perhaps I may be allowed to anticipate Professor Roberts and plead that the rising of the Swedish dukes against Eric XIV in 1568, and his subsequent deposition, should not be overlooked.

This sudden rash of revolts would hardly have come as a surprise to that doughty professional rebel John Knox, who was able to announce reassuringly to Mary Queen of Scots in 1561: 'Madam, your realm is in no other case at this day, than all other realms of Christendom are.'[9] But while contemporaries seem to have felt that they were witnessing the beginnings of a general conflagration, or what Calvin called 'Europae concussio',[10] I am not aware that any historian has grouped them together under the title of 'the general revolution of the 1560s', or has used them as evidence for a 'general crisis of the sixteenth century'.[11]

Perhaps it is not unreasonable to speculate for a moment on the possible reasons for this apparent discrimination in the treatment accorded the seventeenth century. Merriman seems to have been led to his six contemporaneous revolutions partly by his study of seventeenth-century political histories, and partly by the pre-occupation of the 1930s with the possibility of a coming 'world revolution'. He was also influenced by the example of 1848, which gave him the opportunity to draw parallels and comparisons. His principal concern was to consider the relationship of the various revolutions to each other, and his principal conclusion from a study of the 1640s and the 1840s was that 'national rivalries proved stronger than the virus of revolution'—an encouraging conclusion, no doubt, in the circumstances of the 1930s.[12]

Merriman's approach to the seventeenth-century revolutions by way of diplomatic history was of little interest to historians of the post-war generation. But he had bequeathed them a magnificent subject, ready for exploitation. In the context of the post-war historiography of Early Modern Europe, exploitation proved easy enough. The French economist Simiand had taught Early Modern historians to see the sixteenth century as an age of economic expansion, and the seventeenth as a century in which expansion was first halted, and then, around 1650, succeeded by a slump. Given the existence of a major reversal of economic trends in the middle years of the century, Merriman's contemporaneous revolutions seemed both relevant and suggestive. Here, surely, were the social and political manifestations of a crisis affecting the entire European economy. Had his revolutions been those of the 1560s rather than those of the 1640s—the products of an age of expansion rather than of an age of contraction—they might have attracted less attention. Yet even assuming that we can legitimately speak

of a general crisis of the European economy in the mid-seventeenth century—and the evidence, though impressive, is not conclusive—it seems odd that the assumed relationship between economic crisis and political revolution has gone unquestioned. Why should we ignore for the seventeenth century de Tocqueville's perception that revolution tends to come with an improvement rather than with a deterioration of economic conditions?

But the decisive element in the concentration of interest on the revolutions of the 1640s is clearly the supreme importance attributed to the Puritan Revolution in England, as the event that precipitates the collapse of Europe's feudal structure and the emergence of a capitalist society. If the Puritan Revolution is seen as the essential prelude to the Industrial Revolution, it is obvious that a constellation of revolutions benefiting from its reflected glory is likely to outshine any other in the revolutionary firmament. This, at least, seems to be the attitude of the Soviet historian, Boris Porshnev. His Fronde is a bourgeois revolution *manquée*. 'It was,' he writes, 'a French variant of the English bourgeois revolution which was breaking out on the other side of the Channel, and a distant prologue of the French Revolution of the eighteenth century.' He presents the sixteenth-century civil wars, on the other hand, as a combination of feudal quarrels and popular insurrections.[13] Yet, given the upsurge of revolt in the towns of the Ligue in 1588, it is not easy to see any intrinsic reason why the French civil wars should not also be categorized as a bourgeois revolution *manquée*. But perhaps the Ligueurs lacked a progressive ideology.

A contest in the revolution stakes between the 1560s and the 1640s does not seem in itself a particularly profitable enterprise. But it does give rise to a larger and more important question—the question of our general conception of revolution, and its applicability to the study of Early Modern Europe. Here the distinction between Marxist and non-Marxist historian dwindles in importance. The language of our age is pervasive—so pervasive that Professor Mousnier, after cogently criticizing Professor Porshnev for his interpretation of the Fronde in terms of class conflict, can refer to the English civil war, in his most recent book, as 'perhaps the first great bourgeois revolution of modern times'.[14]

Coming from the pen of a historian whose approach to the history of his own country is as staunchly anti-Marxist as that of Professor Mousnier, these words hint at the existence of what seems to be a central problem in the history of Early Modern European insurrections. We are all of us the children of our age, but in this particular field of historical writing, the tricks of time have proved to be more than usually deceptive. Between us and Early Modern Europe lies the late eighteenth century, dominated for us by two events that seem to have done more than anything else to shape our own civilization—the French Revolution, and the Industrial Revolution in England. During the nineteenth century, each of these became a paradigm—an exemplar, in one instance, for political and social development, and in the other for economic development. The twentieth century has appropriated these paradigms from its predecessor, and continues to make use of them as best it can.

How far the current paradigm of the French Revolution actually corresponds to what occurred in the course of that Revolution has been a matter for fierce debate. But the paradigm has not been confined to the French Revolution and the insurrections that have succeeded it. Consciously or unconsciously, nineteenth- and twentieth-century historians have looked at revolts in Early Modern Europe in the light of the late eighteenth-century revolutions, and of their assessment of them. This has frequently provided them with valuable insights into the origins of great events; but the very fact that they applied to many of these Early Modern revolts the word 'revolution' suggests the possibility of unconscious distortions, which may itself give us some cause for unease.

It is true that 'revolution' was by no means an unknown word in sixteenthand seventeenth-century Europe, as applied to upheavals in states. A Spaniard looking back in 1525 on the revolt of the Comuneros expressed his fear of a 'revolution of the people';[15] and in 1647 and 1648 two Italians, Giraffi and Assarino, published accounts of recent insurrections, which they entitled 'The Revolutions' of Naples and Catalonia.[16] But a close study of the concept of 'revolution' by Professor Karl Griewank has shown how slowly, and with what uncertainty, the idea of revolution was brought down from the heavens of Copernicus and applied with any precision to the mutations of states.[17] Sedition, rebellion, *Aufstand*, mutation, revolt, revoltment (John Knox)[18]—these are the words most commonly employed in sixteenth-century Europe. Gardiner's Puritan Revolution was Clarendon's Great Rebellion. Only towards the end of the eighteenth century, under the impact of events in America and France, did 'revolution' effectively establish itself in the European political vocabulary, and acquire those connotations by which we recognize it today.

These include the idea of a violent, irresistible and permanent change of the political and constitutional structure; a powerful social content, through the participation of distinctive social groups and broad masses of the people; and the urge to break sharply with the past and construct a new order in accordance with an ideological programme.[19] Modern historians, accustomed to expect these ingredients of a revolution, have instinctively sought to detect them in Early Modern revolts. Presuming the existence of social protest and class conflict, they have duly found them in the uprisings of the populace. Conditioned to look for minority parties scheming to subvert the state by violence, they have anatomized with great skill the techniques of revolutionary organization. Expecting of a revolution that it should have an innovating ideology, they have effectively isolated and explored the aspirations of those who sought to establish a new order on earth. The work which has been done along these lines has proved immensely fruitful. It has made us aware of motives and forces behind the movements of unrest which were largely veiled from the participants. It has told us things which we could never have known, or could have glimpsed only obscurely, about the patterns of political and social cohesion and the underlying causes of failure or success.

But it would be foolish to ignore the possibility that, in using a concept of revolution that is relatively recent in origin, we may unconsciously be introducing anachronisms, or focusing on certain problems that accord with our own preoccupations, at the expense of others, which have been played down or overlooked. Some recognition of this is implied in recent discussions and debates, particularly on the question of the applicability of the idea of class conflict to Early Modern European society.[20] Although Professor Mousnier has insisted against Professor Porshnev that the popular uprisings in Richelieu's France were fomented by the upper classes and testified to the closeness of the relationship between the peasants and their lords, it would be unwise to disregard the evidence for the existence of fierce social antagonisms in Early Modern Europe. These found expression at moments of unbearable tension—whether in the fury of the Neapolitan mobs in 1585,[21] or in the assault of the Catalan peasants and populace on the nobles and the rich in the summer of 1640.[22]

But it is one thing to establish the existence of social antagonisms, and another to assume that they are the principal cause of conflict. The Catalan rebels first attack royal officials and royal troops; and it is only after disposing of them that they turn on their own ruling class. A revolt may frequently have started, as in Catalonia, against the agents of the State, and then have been transformed into a war on the rich But the parallels between this and a modern class conflict cannot automatically be taken for granted, if only because the ordering of society in Early Modern Europe tended to militate against class solidarity. A society grouped into corporations, divided into orders, and linked vertically by powerful ties of kinship and clientage cannot be expected to behave in the same way as a society divided into classes. Intense rivalries between guildsmen and non-guildsmen, and between the guilds themselves, helped to disrupt community action in urban revolts;[23] and it hardly seems a coincidence that one of the rare examples of a fair degree of urban solidarity is provided by the Comuneros of Castile, where guild organization was weak.

The applicability of the modern notion of ideology to Early Modern revolts seems equally open to question. If by ideology we mean 'a specific set of ideas designed to vindicate or disguise class interest',[24] the uncertainties about 'class' in Early Modern Europe must also be extended to 'ideology'. If we employ it more loosely to mean simply the programme of a particular movement (and this is presumably the way in which it is employed by most Western historians), there still remain large unanswered questions about the extent to which it faithfully represents the character of the movement as a whole. To talk, for instance, of Calvinism as the ideology of the Dutch rebels is to ascribe to the rebellion as a whole a series of ideals and aspirations that we know to be those of only a small minority—and a minority whose importance may well have been inflated, simply because they are the group whose ideals correspond most closely to our notions of what an ideology should be.

Perhaps our principal expectation of a revolutionary ideology is that it should break with the past and aspire to establish a new social order. In a society

dominated, as Early Modern European society was dominated, by the idea not of progress, but of a return to a golden age in the past, the best hope of finding an ideology of innovation lies in certain aspects of the Christian tradition. In particular, the chiliastic doctrines of later medieval Europe look forward to the coming of a new age on earth—the age of the Holy Ghost, characterized by a new social and spiritual order. Up to a point, therefore, it is possible to see the Bohemian Taborites of the fifteenth century as belonging to the tradition of revolutionary innovation by means of violent action. It is certainly arguable that the Taborites did in practice establish for themselves a society in which new forms of social and political organization predominated over the old.[25] But, on the other hand, the Taborites did not reject the traditional threefold ordering of society; and although they were attempting to establish a new spiritual order on earth, the character of this order was determined by reference to the past—in this instance to the primitive church.

The same kind of difficulties are likely to bedevil attempts to bring religiously inspired movements of the sixteenth century into the category of ideological innovation. The peasant movements in early sixteenth-century Germany, for all their millenarian and egalitarian aspirations, were still dominated by the desire to return to a past order, which was held to be eternally valid.[26] The same would also seem to be true of the Calvinist ideal of the advancement of the kingdom of God—an advancement that was anyhow to be achieved by the winning of the state authorities to the cause, rather than by the action of the revolutionary masses.[27]

Even if we dignify—or debase—these religious aspirations with the name of ‘ideology’, it would be misleading to see them as providing a programme of action appealing to the majority of the participants in Early Modern revolts. The Taborites, the Anabaptists and the Calvinists all singularly failed to win anything like universal acceptance of their ideas; and it is not clear why we should regard them as speaking with the true voice of the movements to which they belong, unless it is because they happen to be the most articulate. With our ears straining to catch one particular theme, there is always a danger that, amidst the general uproar, other notes and other voices will go unheard. This danger is not always recognized with such clarity, or expressed with such candour, as it has been by Mr Michael Walzer, in his reference to the English sectaries: ‘However important they are to latter-day genealogists, the sects (even, the Levellers) are of very minor importance in seventeenthcentury history.’[28]

Doubts of this kind might profitably be extended. A fuller recognition of the degree to which our own thinking about revolutions is affected by preconceptions derived from the nineteenth century might at least enable us to isolate more effectively those points at which distortions are most liable to occur. If we accept this possibility, a number of uncomfortable questions about our method and our approach may suggest themselves. I have already hinted at one such question: how far can historians accustomed to look for *innovation* among revolutionaries enter into the minds of men who themselves were obsessed by

renovation—by the desire to return to old customs and privileges, and to an old order of society? How far, too, has our preoccupation with violence as an essential ingredient of revolution concentrated attention on the agitators and the organizers, at the expense of the more passive and the less committed? For all the brilliance of Calvinist organization in the Netherlands, it is arguable that the fate of the revolt was determined elsewhere—by the great mass of people whose religious affiliation was lukewarm or indeterminate,[29] and by those stolid burghers of Holland and Zeeland who edged their way with such extreme caution along the precipitous path that divided loyalty from rebellion.

Most of all, it is open to question whether our persistent search for 'underlying social causes' has not led us down blind alleys, and has concealed from us more profitable ways of approach. I speak here, as in so much else, as one of the errant wanderers. Not that I would claim to have received some sudden illumination on the road to Damascus. It is simply that the constant reading of modern accounts of sixteenth- and seventeenth-century insurrections is likely in due course to induce a weariness of the spirit, and to provoke a certain critical questioning. While it is clear that all the major upheavals in Early Modern Europe represent a combination of different revolts, animated by different ideals and reflecting the aspirations of different groups, it is less clear why 'social' revolts should be regarded as in some way more 'fundamental'. Nor is it clear why we should be expected to assume that the outbreak of revolt in itself postulates structural weaknesses in society. Political disagreement may, after all, be no more and no less than political disagreement—a dispute about the control and the exercise of power.

An age as acutely attuned as ours to the distress signals of the poor and the starving may be correspondingly less sensitive to the cries of the more fortunate for freedom from arbitrary power. The innumerable peasant revolts—the *soulèvements populaires*—that are now being analysed in such painstaking detail provide a terrifying revelation of the misery in which most of Europe's population lived. But we should not, I believe, be afraid to ask the apparently brutal question: did they make any difference? Or, indeed, *could* they make any difference, in a world in which technological backwardness had at least as much to do with the condition of the populace as exploitation by an oppressive ruling class? And if we conclude that they could and did make a difference, we should then go on to determine the precise areas in which that difference was made.

If we can recognize that contemporary preconceptions about the nature of revolution may have helped to shape our treatment of Early Modern revolts, we are at least in some position to attempt remedial action. Our priorities, for instance, can be set against those of contemporaries, in an attempt to discover which of theirs have receded into the background, or have come to be overlooked. It is a salutary experience to watch the development of the French civil wars through the sharp eyes of Estienne Pasquier, whose evaluation of events is that of an intelligent and well-read sixteenth-century layman. He wrote:

> There are three things of which one should be infinitely afraid in every principality—huge debts, a royal minority, and a disturbance in religion. For there is not one of these three which is not sufficient of itself to bring mutation to a state.[30]

No doubt Pasquier's analysis is inadequate, even by contemporary standards. His historical analogies were essentially political, and he set the unfolding drama of conflict in France into the context of famous faction feuds. He wrote in 1560:

> Two miserable words of faction, Huguenot and Papist, have insinuated themselves amongst us; and I fear that in the long run they will lead us to the same calamities and miseries as the Guelfs and Ghibellines in Italy, and the White and the Red Rose in England.[31]

But this was not an unreasonable assessment of events from the standpoint of 1560; and if he omitted the social and economic considerations—the discontents of the gentry, the social consequences of rising prices—that loom so large in modern accounts of the French wars, this does not necessarily mean that he was unaware of their influence on events. Pasquier and his contemporaries were capable enough of seeing the existence of a relationship between political and social grievance. It was in the degree of significance to be accorded to this relationship that a sixteenth-century approach diverges most sharply from our own. An age that has devoted itself to meticulous research into the fortunes of nobles and gentry is likely to find something almost comically casual about the words that Joachim Hopperus slips into his account of the origins of the Dutch revolt:

> Several of the principal leaders were at this time very heavily burdened with debts. This is sometimes considered a source of unrest and attempted innovation, since such people hope to take advantage of disturbances in the state to re-establish their fortunes.[32]

But it may be that we have been equally casual in our approach to what contemporaries themselves regarded as important: Pasquier's 'royal minorities', for instance, and indeed the whole question of kingship. It is now almost impossible for us to grasp the degree to which changes in the character of kingship affected the dispositions of power in the state. In societies where all the threads of patronage ultimately come to rest in the hands of the king, any of the accidents and hazards to which hereditary kingship is prone are likely to have profoundly disturbing consequences. The apparent turbulence of politics in the 1560s may therefore not be entirely unrelated to a remarkably high mortality rate among monarchs in the preceding decade, and the accession of new and inexperienced rulers, some of whom were women or children. Similarly, if a comparison is to be made between the histories of France and Castile, it does not

seem entirely irrelevant to consider how far Castile's immunity from rebellion after 1521 is to be ascribed to a high degree of social stability, and how far to the accident that it escaped royal minorities and the baneful presence (except for a short period in the reign of Philip IV) of adult cadet princes.[33]

There is, however, another area, in which most modern historiography seems to have been even less at home, and with far more considerable consequences. The search for the causes of discontent is nowadays more likely to lead to religious or social grievance than to a sense of national loyalty. Yet the apparent uncertainty of modern historians when faced with the question of nationalism in Early Modern Europe stands in marked contrast to the increasingly confident use in the sixteenth century of the words *patria* and *patrie*. When the Corsican leader, Sampiero Corso, turned to Catherine de Medici for help in the early 1560s to free his native island from Genoese domination, she presented him with a number of banners bearing the heroic inscription: *pugna pro patria*.[34] The rebels of Ghent in 1578 not only spoke of defending their *patrie*, but also referred to themselves *as patriotes*.[35] The lawyers and judges in the reign of Charles I gained a reputation with parliament of being 'good patriots';[36] and Masaniello, the hero of the Neapolitan revolt of 1647, was hailed as *liberator patriae*.[37]

It is possible that, in approaching these apparent manifestations of patriotic sentiment, we have again been both influenced and inhibited by our nineteenth-century inheritance. The Commonwealth', wrote Lord Acton, 'is the second stage on the road of revolution, which started from The Netherlands, and went on to America and France.'[38] For all the qualifications introduced by Acton himself, there was a strong temptation to look at Early Modern revolts through the lens of the French Revolution, interpreted this time in accordance with the liberal-national tradition. The modern reaction against the historiographical excesses of this tradition is natural enough. But the manifestations of some kind of community consciousness in Early Modern revolts are too numerous and too forceful to allow the question of nationalism to be left in a kind of historical limbo.

There are obvious difficulties about attempting to equate these various manifestations with a nationalism of the nineteenth-century variety. All too often a supposed allegiance to a national community turns out, on inspection, to be nothing of the kind. The *patria* itself is at least as likely to be a home town or province as the whole nation,[39] and a revolt, like that of the Dutch, that is represented in nineteenth-century historiography as a nationalist uprising may just as convincingly be depicted as a manifestation of particularist, rather than nationalist, sentiments.[40]

Yet even though *patria* might apply in the first instance to a native city, it could at times be extended, as in the Castile of the Comuneros,[41] to embrace the entire community of the realm. But whether the community was local or national, expressions of allegiance to it assumed the same form: a deep and instinctive antipathy to outsiders. Throughout the Early Modern period, this antipathy was a powerful driving force behind popular revolt. It moved the Corsican peasants in

the 1560s to take up arms against the Genoese, and the Catalan peasants in 1640 to take up arms against the Castilians. *Visca la terra*—long live the land!—is the perennial cry of the Catalan populace as it turns out, to the summons of the church bells, to attack the bands of Castilian soldiers making their way to their embarkation point at Barcelona.[42]

This popular nationalism figures prominently in the accounts of revolts written by nineteenth-century historians, who were themselves so often the products of a Romantic culture nurtured on the legends and the songs with which the deeds of the rebels were kept alive in folk memory. But in idealizing it, they helped to discredit it, and oversimplified a complex phenomenon. What was often, at its least attractive, no more than an instinctive hatred of outsiders, was transmuted into a self-conscious identification with a national community, embodying certain specific ideals. But the Romantic historians were not totally mistaken in assuming the existence in Early Modern Europe of some such sense of identification, although they may have expected too often to find it expressed even at the very lowest social levels. For alongside the more obvious manifestations of popular sentiment, there was also to be found another phenomenon, which has yet to receive the attention and the analysis it deserves. This might perhaps best be described as a corporate or national constitutionalism; and while it may have reached down, in some form, to the lower levels of society, it was essentially the preserve of the dominant social and vocational groups in the State—nobles and gentry, urban patriciates, the lawyers, the clergy, the educated.

Perhaps it may be defined as an idealized conception of the various communities to which allegiance was owed; and it embraced, in ever-widening circles, the family and vocational community to which they belonged, the urban or provincial community in which they lived, and, ultimately and sometimes very hazily, the community of the realm. This idealized conception of the community was compounded of various elements. There was first, and most naturally, the sense of kinship and unity with others sharing the same allegiance. But there was also a sense of the corporation or community as a legal and historical entity, which had acquired certain distinctive characteristics with the passage of time, together with certain specific obligations, rights and privileges.

The community was founded on history, law and achievement, on the sharing of certain common experiences and certain common patterns of life and behaviour. As such, it was an ideal—indeed, an idealized—entity, already perfect in itself. It was, though, for ever subject to attacks from enemies, and to erosion at the hands of time. The highest obligation incumbent upon its members was therefore to ensure that in due course it should be transmitted intact to their successors. The plea for the faithful fulfilment of this obligation echoes right through the history of Early Modern Europe, from the Florentine who urged his fellow-citizens in 1368 to ‘leave to posterity that which was left to us by our ancestors’,[43] to the Catalan canon who begged his brother canons in 1639 not to ‘let us lose in our own time what our forbears have so bravely won’.[44]

The sixteenth century seems to have contributed a new sophistication and a new awareness to the sacred task of defending a community whose rights and liberties were embodied in written constitutions and charters and kept alive in the corporate memory. In particular, it engaged with enthusiasm in legal and historical research. The great revival of interest in the customary law—a revival symbolized in France by the names of Bodin and Hotman[45]—not only provided new defences against arbitrary power decked out with the trappings of Roman law, but also helped to establish the idea that each nation had a distinct historical and constitutional identity.[46] By endowing the community with a genuine or fictitious constitution, set firmly into a unique historical context, the sixteenth-century antiquarian movement gave new meaning to the struggle for the preservation of liberties. The corporation, the community, the *patria* all acquired a firmer identity as the historical embodiment of distinctive rights.

The idea of the *patria* was also fostered by the new humanist education. A governing class which had imbibed the history of Greece and Rome from an early age would have no great difficulty in making an identification between its own idealized community and the politics of classical antiquity.[47] Hobbes says in the *Behemoth:*

> There were an exceeding great number of men of the better sort, that had been so educated, as that in their youth having read the books written by famous men of the ancient Grecian and Roman commonwealths concerning their polity and great actions; in which books the popular government was extolled by the glorious name of liberty, and monarchy disgraced by the name of tyranny; they became thereby in love with their forms of government. And out of these men were chosen the greatest part of the House of Commons.
>
> The core of rebellion, as you have seen by this, and read of other rebellions, are the Universities…
>
> The *Universities* have been to this nation, as the wooden horse was to the Trojans.[48]

Until a great deal more research has been done on education in Early Modern Europe, it is impossible to determine what degree of importance should be attached to Hobbes's angry denunciations. But the intellectual influences that went to shape the conception of their own community among the governing classes of Europe are obviously a matter of the greatest interest, since it was the idealized community or the *patria* that gave them the frame of reference by which they determined their own actions and assessed those of others. In his book on *The Community of Kent and the Great Rebellion,* Professor Everitt has shown how the political behaviour of the dominant groups in Kentish society between 1640 and 1660 can only be understood in the light of their intense devotion to an idealized local community. While the gentry of Kent included convinced royalists and parliamentarians among their number, the principal aim of the

majority was to 'stand for the defence of the liberties of their unconquered nation' against assaults from either camp.[49]

This devotion to an idealized community can be paralleled all over Europe, on a local, a regional and a national scale. Everywhere, the instinct of the ruling classes was to preserve a heritage. While in some instances this heritage might become indissolubly bound up with religious loyalties, the preservation of a heritage seems to have outweighed every other cause, including that of religion, in its appeal to the majority of the ruling nation. The Lords of the Congregation wrote to the nobility, burghs and community of Scotland in 1559:

> If religion be not persuaded unto you, yet cast ye not away the care ye ought to have over your commonwealth, which ye see manifestly and violently ruined before your eyes. If this will not move you, remember your dear wives, children and posterity, your ancient heritages and houses.[50]

Eloquent appeals for action in defence of laws and liberties were obviously likely to carry additional conviction when the arbitrary power that threatened them was also an alien power. In Scotland, Corsica and the Netherlands in the 1560s, in Catalonia and Portugal in the 1640s, the rebels found it easier to rally support, because the oppression came from foreign rulers, foreign officials and foreign troops on native soil. In these circumstances, a revolt originally sparked by religious protest or sectional discontents was capable of gathering support and momentum by combining in a common patriotism the constitutionalism of the privileged classes and the general antipathy to the outsider felt by the population at large.

This combination almost always proved fragile and transitory, because the idea of a national community to which all sections of society owed their prime allegiance was still so weakly developed. The national community was shot through with rival allegiances, and riven by sectional and social hatreds. Moreover, the constitutionalism of the privileged was all too often no more than a convenient device for defending the interests of an exclusive caste on the basis of bogus history and bogus law. Yet, in recognizing this, one must also recognize that the defence of liberties could, in certain circumstances, broaden into the defence of liberty; and that the pursuit of sectional advantage was not necessarily incompatible with the furtherance of a genuinely constitutional cause. For all its obvious deficiencies, constitutionalism provided the political nation with an ideal standard against which to measure current realities. Once this ideal standard existed, it was always capable of extension by a leader of political genius. The *patrie*, as glimpsed by William of Orange, was something more than a society in which the rights and liberties of the privileged were safe from the exercise of arbitrary power. It was also a society that included freedom of conscience among its liberties; an essentially open society, in which men were free to come and go and educate themselves without restrictions from above.[51]

Given the existence of an idealized vision of the community, however restricted that vision, movements of protest are likely to occur within the political nation when the discrepancy between the image and the reality comes to seem intolerably wide. In Early Modern Europe it is these movements of protest from above, and not the popular uprisings, that are capable of leading to a 'mutation in the State'. The problem, though, is to relate them to other manifestations of discontent, simultaneous or complementary, which have their origin in religious, fiscal or social grievances among the general population. 'Then is the danger,' as Bacon appreciated, 'when the greater sort do but wait for the troubling of the waters among the meaner, that then they may declare themselves.'[52] But it is impossible to establish here any common pattern of revolt. In the Netherlands of the 1560s an aristocratic movement benefited from simultaneous movements of religious and patriotic protest. But the aristocratic movement was halted in its tracks by the popular uprising—the iconoclastic fury—of August 1566. In Catalonia in 1640 the popular uprising, provoked by the behaviour of foreign troops, encouraged the leaders of the political nation to seize the initiative, and to transform a long-standing movement of protest into a decisive break with the Crown. In the England of the 1640s, it was only *after* the political nation had seized the initiative, and then itself split down the middle, that the people began to move. In Naples in 1647 the popular movement failed to evoke an effective response among the dominant social classes, and doomed itself to destruction.

In states displaying such varieties of political and social organization great variations in the pattern of revolt are only to be expected. There will always be men bold enough, angry enough, or frightened enough to seize the opportunity afforded by an upsurge of popular fury or by a sudden weakening of the state. The crucial question then becomes the attitude adopted by the mass of the uncommitted among the ruling class. Will they rally behind the Crown and the agents of royal authority in an emergency, or will they allow the leaders of the insurrection to have their way? The answer is likely to depend on a delicate balance between the ruling class's persistent fear of social upheaval, and its feeling of alienation from the Crown. In the Netherlands in 1566, for example, the political nation rallied to the government of Margaret of Parma when social upheaval threatened it. But in 1572, after five years of repressive government by the Duke of Alba, it had become so alienated from a regime that had launched an assault on its liberties, that it adopted a position of neutrality when the emergency came. The same is true of the Catalan political nation in the summer of 1640: the extent to which it had been alienated from Madrid by the policies of Olivares over the previous twenty years was sufficient to prevent it making any serious move to check the course of the revolt. In Naples, on the other hand, the nobles and gentry, for all their discontents, had remained closely associated with the viceregal administration, which had bribed them with favours and privileges because it needed their help in mobilizing the resources of Naples for war. This association made them the immediate objects of popular hatred in 1647; and they

had nothing to gain from breaking with a regime which had shown itself more responsive to their interests than any that was likely to replace it.[53]

If the 1560s and the 1640s prove to be decades of more than usual unrest in Europe, it does not seem a coincidence that they were both periods in which the traditional loyalty of ruling classes to their princes had been subjected to very considerable strain. In both decades, several states were still engaged in, or were only just emerging from, a long period of warfare which had imposed heavy demands on national resources. In both decades, too, there was deep discontent among the ruling classes over the prevailing style of government. In the 1560s resentment was focused in particular on the rule of secretaries and professional civil servants: Cecil in England, Persson in Sweden,[54] Granvelle in the Netherlands. In the 1640s, it was focused on the rule of favourites —Strafford, Richelieu, Olivares—all of whom had shown a degree of political ruthlessness which was all the more objectionable because, as nobles, they had been traitors to their kind.

At a time when there was already something of a coolness between the Crown and the political nation, the situation was aggravated in both periods by signs of unusually energetic activity on the part of the state. In the late 1550s or early 1560s, the state's preoccupation with religious dissidence as a threat to its own authority had made it exceptionally vigorous in its employment of counter-measures. These brought the central power into conflict with sectional interests and those of local communities, and aroused widespread disquiet about the infringement of rights and liberties. In the 1630s and 1640s the main thrust of state power was fiscal rather than religious, but the consequences were not dissimilar. The financial demands of the state brought it into direct conflict with important sections of the political nation, which expressed its discontent through its representative institutions, where these still existed, and through the tacit withdrawal of allegiance.

In these circumstances, a group of determined rebels is well placed to make the running. The Crown, and those sections of the governing class immediately dependent upon it, finds itself temporarily isolated. The privileged and propertied classes hold aloof, or lend their sympathy and support to the rebels. But in practice the rebels have very little time at their disposal. Not only do their actions give rise to new feuds and vendettas, but a society that thinks essentially in terms of restoration is likely to baulk at measures that smack of innovation. 'From the beginning of the rebellion,' wrote Hobbes, 'the method of ambition was constantly this: first to destroy, and then to consider what they should set up.'[55] Rebels who contrived to give this impression were bound to alienate the body of uncommitted but conservative opinion in a political nation that was anyhow terrified that its own internal disputes would place power in the hands of the populace.

Rebels, therefore, could not count on continuing support from within the ruling class. Their movement, too, was likely to have only a narrow social base in a vertically articulated society. In the circumstances, they were bound to be

driven back on alternative sources of help—and these could only come from outside. Merriman, with world revolution at the back of his mind, was impressed by the lack of co-operation between rebel regimes in different states. But much more impressive is the extent to which rebels sought, and secured, foreign assistance at vital stages in their revolts. Foreign aid, in fact, seems to have been an indispensable requirement for any revolt, if it were to have a chance of perpetuating itself. It was English military assistance that enabled the Scottish rebels to triumph in 1560. It was the French, the Germans and the English who saved William of Orange and the Dutch. It was the support of foreign Protestant or Roman Catholic powers that gave an additional lease of life to the rebel factions in France. In the 1640s the story was the same. The Scots came to the help of the English, the French to the help of the Catalans, the French and English to the help of the Portuguese.

The dependence of Early Modern revolts on external assistance suggests something of their character and their limitations. Sometimes they were furthered, sometimes impeded, by popular uprisings; but these were ephemeral movements, which could achieve little or nothing without assistance from groups within the ruling class. The prime aim of this class was to conserve and restore; and this aim at once determined the scope of the rebels' action, and the extent of their support. A ruling class alienated from the Crown by encroachment upon its liberties was prepared to let royal authority be challenged, and this allowed the rebels such successes as they in fact achieved. But once the heritage had been saved, the political nation reverted to its traditional allegiance, and those rebels who chose to persist in rebellion were compelled to look abroad for help.

The sixteenth and seventeenth centuries did indeed see significant changes in the texture of European life, but these changes occurred inside the resilient framework of the aristocratic-monarchical state. Violent attempts were made at times to disrupt this framework from below, but without any lasting degree of success. The only effective challenge to state power, and to the manner of its exercise, could come from within the political nation—from within a governing class whose vision scarcely reached beyond the idea of a traditional community possessed of traditional liberties. But this proved to be less constricting than it might at first sight appear. Renovation in theory does not of itself preclude innovation in practice; and the deliberate attempt to return to old ways may lead men, in spite of themselves, into startlingly new departures. There remained, too, sufficient room for the ruling class to be able to challenge the state at the two points where its activities were most likely to influence the character of national life. By resisting the state in the matter of taxation, it might destroy, or prevent the establishment of, a major obstacle to economic development; and by resisting its claims to enforce religious uniformity, it might remove a major obstacle to intellectual advance. If significant change came to certain European societies in the sixteenth and seventeenth centuries, it came because this challenge was effectively carried through.

By the eighteenth century, the growing awareness of man's capacity to control and improve his environment would make it more fashionable than it had been in the seventeenth century to think in terms of innovation. At this point the character of revolt would also begin to change; and rebellion might come to assume the characteristics of revolution. Until then, revolts continued to be played out within the context of the ambitions of the state, on the one hand, and the determination of the dominant social groups to preserve their heritage, on the other. If this determination came to be expressed in an increasingly sophisticated language, this was because the political nation itself was becoming more sophisticated. National constitutionalism learnt the language of law, of history and antiquity. Perhaps, then, it is to the rise of a literate and educated lay establishment, not to the rise of new social classes, that we should look if we are to understand the eventually decisive achievement of Early Modern revolts—the transformation of liberties into liberty. That one man, at least, guessed as much, we can see from the dialogue of the *Behemoth:*[56]

B. For aught I see, all the states of Christendom will be subject to these fits of rebellion, as long as the world lasteth.

A. Like enough; and yet the fault (as I have said) may be easily mended, by mending the Universities.

NOTES

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1 Many of the contributions to the debate are to be found reprinted in Trevor Aston (ed.), *Crisis in Europe, 1560–1660* (London, 1965). For expressions of dissent, see E.H.Kossmann, 'Trevor-Roper's "general crisis"', *Past and Present,* XVIII (November, 1960), pp. 8–11; A.D.Lublinskaya, *French Absolutism: The Crucial Phase, 1620–1629* (Cambridge, 1968) and I.Schöffer, 'Did Holland's Golden Age coincide with a period of crisis?', Chapter 4 in this volume. Dr Schöffer's admirable article, of which I was unaware when I originally drafted this essay, makes a number of points that coincide closely with my own.

2 R.Mousnier, *Les XVIe et XVIIe Siècles* (Paris, 1954).

3 Aston, op. cit., pp. 5–58.

4 Ibid., pp. 59–95.

5 Ibid., p. 59.

6 Ibid., p. 221.

7 R.B.Merriman, *Six Contemporaneous Revolutions* (Oxford, 1938), p. 89.

8 Aston, op. cit., pp. 62 and 63.

9 *John Knox's History of the Reformation in Scotland,* ed. W.C.Dickinson, I (London, 1949), p. 367.

10 R.Nürnberger, *Die Politisierung des Französichen Protestantismus* (Tübingen, 1948), p. 91 n. 57.
11 The links between the Huguenot and the Dutch revolts have, of course, received considerable attention, especially from nineteenth-century historians. Cf. in particular Kervyn de Lettenhove, *Les Huguenots et Les Gueux*, 6 vols (Bruges, 1883– 5). See also H.G.Koenigsberger, 'The organization of revolutionary parties in France and the Netherlands', *Journal of Modern History;* XXVII (1955), which draws interesting parallels between the organization of the revolts in France, the Netherlands and Scotland. See also p. 9 in this volume.
12 Merriman, op. cit., pp. 209 and 213.
13 Boris Porchnev (Porshnev), *Les soulèvements populaires en France de 1623 à 1648* (Paris, 1963), pp. 537, 17, 47.
14 R.Mousnier, *Peasant Uprisings* (London, 1972), p.xvii.
15 J.A.Maravall, *Las Comunidades de Castilla* (Madrid, 1963), pp. 243–4.
16 A.Giraffi, *Le Rivolutioni di Napoli* (Venice, 1647); Luca Assarino, *Le Rivolutioni di Catalogna* (Bologna, 1648). See also Vernon F.Snow, The concept of revolution in seventeenth-century England', *The Historical Journal*, V (1962), pp. 167–74.
17 K.Griewank, *Der Neuzeitliche Revolutionsbegriff* (Weimar, 1955).
18 Knox, op. cit., I, p. 193.
19 Griewank, op. cit., p. 7.
20 See Mousnier, *Peasant Uprisings*, pp. 18–19 and also p. 320 n. 9 for reference to his debate with Porshnev. Further discussion of the question of class in Early Modern society may be found in R.Mandrou, *Introduction à la France moderne* (Paris, 1961), pp. 138–64, and F.Mauro, *Le XVIe Siècle européen, aspects économiques* (Paris, 1966), pp. 337–44.
21 R.Villari, *La Rivolta antispagnola a Napoli* (Bari, 1967), p. 44.
22 J.H.Elliott, *The Revolt of the Catalans* (Cambridge, 1963), pp. 462–5.
23 For a classic example, see Gene A.Brucker, *Florentine Politics and Society, 1347–1378* (Princeton, NJ, 1962), p. 55.
24 A.Gerschenkron, *Continuity in History and Other Essays* (Cambridge, Mass., 1968), p. 65.
25 H.Kaminsky, *A History of the Hussite Revolution* (Berkeley, Calif, 1967), pp. 481ff.
26 Griewank, op. cit., p. 102.
27 Nürnberger, op. cit., pp. 19–21.
28 M.Walzer, *The Revolution of the Saints* (London, 1966), p.x.
29 See J.W.Smit, 'The present position of studies regarding the revolt of the Netherlands', in J.S.Bromley and E.H.Kossmann (eds), *Britain and the Netherlands* (London, 1960), pp. 23–5.
30 E Pasquier, *Lettres historiques (1556–1594)*, ed. D.Thickett (Geneva, 1966), p. 100.
31 Ibid., p. 47.
32 A.Wauters (ed.), J.Hopperus, 'Recueil et mémorial des troubles des Pays Bas', in *Mémoires de Viglius et d'Hopperus sur le commencement des troubles des Pays Bas*, (Brussels, 1868), p. 237.
33 It is significant that Olivares was greatly exercised by the problem of how to educate and employ the Infantes Don Carlos and Don Fernando, and complained of the lack of precedents to guide him: British Library, Egerton MS. 2081 f.268, *Papel del Conde Duque sobre los Infantes*.

34 A.De Rublé, *Le Traité de Cateau-Cambrésis* (Paris, 1889), p. 77.
35 G.Malengreau, *L'Esprit particulariste et la révolution des Pays-Bas au XVIe siècle* (Louvain, 1936), p. 82 n. 4.
36 T.Hobbes, *Behemoth*, ed. F.Tönnies (London, 1889), p. 119.
37 A.Giraffi, *An Exact Historie of the Late Revolutions in Naples* (London, 1650), p. 160.
38 Lord Acton, *Lectures on Modern History* (London, 1907), p. 205.
39 Cf. the description of Barcelona as his *pàtria* by Pujades (Elliott, op. cit., p. 42).
40 As it is by Malengreau, op. cit.
41 Maravall, op. cit., p. 55. See also G.Dupont-Ferrier, 'Le sens des mots "patria" et "patrie" en France au Moyen Age et jusqu'au début du xvii^{e} siècle', *Revue historique*, CLXXXVIII (1940), pp. 89–104.
42 Elliott, op. cit., p. 253.
43 Brucker, op. cit., p. 396.
44 Elliott, op. cit, p. 344.
45 Julian H.Franklin, *Jean Bodin and the Sixteenth-Century Revolution in the Methodology of Law and History* (New York, 1963), especially Chapter 3.
46 Ralph E.Giesey, *If Not, Not* (Princeton, N J. 1968), p. 245. On the general question of constitutionalism, see in particular J.G.A.Pocock, *The Ancient Constitution and the Feudal Law* (Cambridge, 1957) and Michael Roberts, 'On aristocratic constitutionalism in Swedish history', *Essays in Swedish History* (London, 1967). I have also greatly benefited from discussions on this subject with Mr Quentin Skinner of Christ's College, Cambridge, who kindly read an early draft of this essay and made valuable comments on it.
47 For the influence of classical polities on seventeenth-century English thought, see Zera S.Fink, *The Classical Republicans* (Evanston, 111., 1945).
48 Hobbes, op. cit., pp. 3, 40, 58.
49 A.Everitt, *The Community of Kent and the Great Rebellion* (Leicester, 1966), p. 269.
50 Knox, op. cit., I, p. 225.
51 *Apologie ou defense de… Prince Guillaume* (Leyden, 1581), especially p. 91.
52 'Of seditions and troubles', *The Works of Francis Bacon*, ed. J.Spedding, VI (London, 1858), p. 411.
53 The extent of the Crown's concessions to the Neapolitan nobility, and the political consequences of this policy, emerge very clearly from Villari, op. cit.
54 Michael Roberts, *The Early Vasas* (Cambridge, 1968), p. 224.
55 Hobbes, op. cit., p. 192.
56 Ibid., p. 71.

6
THE PRECONDITIONS OF REVOLUTION IN EARLY MODERN EUROPE
Did they really exist?*

A.Lloyd Moote

Despite our general understanding of the wave of Early Modern European revolts that began with the Wars of Religion after 1560 and ended with a cluster of political rebellions during the 1640s, we still do not know to what extent and in what ways those upheavals constituted a revolutionary situation. Here is an excellent laboratory for comparative history by the traditionalist scholar daring enough to escape old generalizations about the uniqueness of this or that revolt. Here is also a mass of useful data for social scientists who specialize in testing revolutionary models. Yet until quite recently traditionalist historical scholarship has been content with brief critiques or bland acceptance of Trevor-Roper's thesis of a mid-seventeenthcentury 'general crisis',[1] while sociologists and political scientists have generally applied their model-building technique to the period since the late eighteenth century, which boasted political changes that were more intelligible to twentieth-century man than those of 1560–1660.[2]

Now, some of this timidity and evasiveness has been swept aside. Early Modern historians have not yet found a Robert Palmer to give us another 'Age of Revolution', but Helmut G.Koenigsberger has produced three essays that pull together his long-standing views on revolution and crisis in the states influenced by the Spanish Habsburgs between 1516 and 1660.[3] His traditionalist approach is complemented by a multi-authored book in which five distinguished scholars discuss particular uprisings of the period 1560–1775 from the perspective of what social scientists call the 'preconditions' of revolution. The editors of this innovative work, Robert Forster and Jack P. Greene, have made their own contribution by extracting from the discussions of specific uprisings some provocative generalizations about Early Modern revolt and revolution. In their introductory essay, the editors categorize the uprisings according to 'types', and sort out the overall preconditions of revolution that lie behind those types that seem to be revolutionary in scope.[4] Even a cursory reading of both books reveals that the fundamental questions raised by the authors and editors cannot be discussed adequately in a brief, conventional review. Some sort of extended analysis seems in order.

The most obvious question concerns the very meaning of that troublesome word 'revolution'. Professor Koenigsberger takes it for granted that truly revolutionary movements were involved in the rise of Dutch, Scottish, French

Leaguer, and French Huguenot 'parties' during the late sixteenth century. He goes on to describe the Bohemian phase of the Thirty Years' War as a 'revolution', and he labels the movements that followed in France, England, the Swiss Cantons, Naples, Palermo, Catalonia, Portugal, Sweden, Poland and the Dutch Republic as 'revolutions and coups'. Not to be outdone by Trevor-Roper, the author expands the mid-seventeenth-century political crisis into what he calls 'a genuine crisis of societies and of their political constitutions', and adds his own 'great political crisis of the mid-sixteenth century'.[5] The very title of the Forster-Greene volume implies what Koenigsberger assumes by its assertion that there were 'Preconditions of Revolution in Early Modern Europe'. And the first two essays of that multi-authored work make it clear that preconditions actually led to a 'Dutch Revolution' and an 'English Revolution'. As if to convince those who may believe these historians are merely superimposing a new terminology on what we used to call the Dutch Revolt and the Great Rebellion, Forster and Greene are quite explicit in their meaning of revolution. According to the definition that they have borrowed from a social scientist, revolution is:

> sharp, sudden change or attempted change in the location of political power which involved either the use or the threat of violence, and, if successful, expressed itself in the manifest and perhaps radical transformation of the process of government, the accepted foundations of sovereignty or legitimacy, and the conception of the political and/or social orders.[6]

Of course the wary reader is still tempted to reject the renaming of so many revolts as revolutions or revolutionary movements. Indeed, it is appropriate to object that ideological, social, and political conditions prior to the late eighteenth century were unconducive to revolution, in the sense of some basic, lasting structural change in government or society Especially in the realm of ideas, Europe before the Age of Democratic Revolution was non-revolutionary, looking forlornly back to a lost Golden Age rather than forward to a better life. Such a generalization seems to hold even if we include those waves of millennial movements that combined the Garden of Eden with the Second Coming.

In their efforts to typify political upheavals, social scientists have sensed the existence of this pre-modern mentality and consequently of an ideological dividing line between pre-modern revolt and modern revolution. Note, for example, Chalmers Johnson's scheme. It starts with (a) simple rebellions designed to restore a system betrayed by an entrenched élite, exemplified by a jacquerie such as Pugachev's revolt against the late eighteenth-century Russian state and society. It then turns to (b) advanced rebellions by declining social groups seeking an idealized form of the past system. Finally it turns to two types of revolution, both of which are illustrated exclusively by uprisings from the period since the late 1700s. The first is (c) the simple revolution that goes beyond rebellion in consciously aiming at some basic and unprecedented changes in the

existing system, and which apparently dates from the American Revolution. The second revolutionary type is (d) the total revolution, allegedly involving a conscious break with all existing values, and best illustrated by the French Revolution of 1789.[7] The obvious conclusion is that the assumption of progress implicit in the Enlightenment constituted the ideological breakthrough necessary for turning aside Early Modern mental blocks against revolution, and making types (c) and (d) possible.

In social structure and attitudes, the late eighteenth century seems to have been just as much of a watershed. Its 'élites', in the broad sense of the well-todo whose values and habits were considered the ideal by lower social orders and who had direct or indirect control over governmental policies, were vulnerable to criticism and attack from below on their *raison d'être.* This contrasts with the immediately preceding eras, when blind attacks on the upper orders failed to make much headway against the deeply ingrained ideas of hierarchy and stratification, even in England, where wealth worked more than elsewhere to undermine the importance of status.

Politically, too, there was a shift, though it may have been more gradual. In the sixteenth and seventeenth centuries no state, with the possible exception of England, had progressed far towards centralization. In one country after another, there was lacking a long-established fixed capital for both court and bureaucracy. Hence it was far more difficult for would-be rebels to overthrow and replace a political system than it would be a century later. Most uprisings could aspire to little more than breaking from or changing the personnel of their rather amorphous governments.

It may be mere coincidence that the eighteenth-century specialists, Forster and Greene, deal more directly with the nature of Early Modern revolution than the sixteenth-century-oriented Koenigsberger. In any case, they have established the first typology of Early Modern revolution, and in the process forced us to start asking what distinguishes different upheavals of the time. Descending from the most to the least revolutionary, their types of uprisings are as follows:

(a) Great national revolutions, specifically the Dutch break with Spanish rule in the late sixteenth century and the English overthrow of the Stuarts in the 1640s.
(b) National revolts such as the French Fronde of 1648–53 and the almost contemporaneous Catalan rebellion against Spain, which, according to Forster and Greene, had the potential of becoming genuine revolutions.
(c) Large-scale regional rebellions with limited revolutionary potential, as shown by Pugachev's movement.
(d) Secessionist *coups d'état,* exemplified by the Portuguese overthrow of Spanish rule 1640–68.
(e) Urban jacqueries, such as those in mid-seventeenth-century Sicily and Naples.[8]

The main weakness in Forster and Greene's scheme is that it does not spell out exactly what distinguishes one type of upheaval from another, despite an interesting discussion leading up to their typology. The reader is therefore forced to draw inferences, and he is unfortunately led to the conclusion that quantitative measurements, particularly 'bigness', have determined the Forster-Greene types. That is, the more people, the larger the territory and the greater the violence involved, the more revolutionary an upheaval was. And in so far as this scheme is quantitative, it looks strikingly similar to one by George Pettee, whose social science-oriented typology included the categories of 'great national revolution', 'rebellion in one area against rule by the government of another country' and 'public palace revolution'.[9]

Fortunately it is not necessary to accept either the Pettee or ForsterGreene approach. Other model-builders have managed to offset the weakness of such elusive quantitative yardsticks as body counts, duration and mass participation with qualitative, yet rather precise, criteria. Especially noteworthy is James Rosenau's emphasis on the 'object' of rebel attacks. We historians could well ask, with him, whether particular uprisings had the object of opposing merely (a) the personnel of a regime, (b) its authority, or some institutional base, or (c) its very structure.[10]

The reader should be warned that the actual typologies that Chalmers Johnson and others derive from a combination of quantitative and qualitative criteria are not as useful as the criteria themselves. Probably the main reason for this is that not all of the yardsticks have been used consistently. Johnson does try to take into account the rebels' targets, ideals, and ideologies, as well as their social background. Yet by virtually ignoring the role of social élites and by concentrating on mass movements he renders invalid his criterion of social background.[11] At the present time, it would appear that social scientists cannot integrate more than a few variables.

This imperfection can have serious consequences. In his scheme Johnson uses so few variables that the lines between types of revolt and revolution remain blurred. For example, by admitting that both his 'simple revolution' and 'advanced rebellion' types have the same target—the regime—he begs the question of the distinction between the two. The same sort of confusion bedevils the less complex typology of Forster and Greene. For they establish five types of 'political upheaval', of which only the 'great national revolution' is clearly revolutionary; and then they blur the line between revolution and revolt by placing that revolutionary category together with national revolts and large-scale regional uprisings in a vague grouping of 'actual or potential' revolutions[12]

For all the weaknesses of the Forster-Greene scheme, and of even the most sophisticated typologies by social scientists, it must be emphasized that models can be usefully applied by traditionalist historians to particular upheavals. We all know that the types are imperfect, theoretical concepts, so there is no danger of distorting the facts to fit the theories. And by comparing theory and reality, we can come closer to understanding the nature of actual uprisings, to knowing what

we should compare and contrast when handling more than one uprising, and to defining what really constitutes revolution in the Early Modern period.

In contrast to the Forster-Greene attempt to understand the nature of revolutions by typifying them, Professor Koenigsberger uses the traditionalist approach of bypassing definitions and proceeding to the business of describing movements he assumes were revolutionary. This he does with a remarkable balancing of key facts with generalizations that apply to several revolts. Particularly provocative is his treatment of late sixteenth-century upheaval. He takes into account the Scottish overthrow of Mary and non-religious turmoil in the Spanish and independent lands of the Italian peninsula, while concentrating on the French Huguenots and Catholic Leaguers as well as the Dutch Calvinists and Sea Beggars. The following traits can be ascribed to these movements. First, they involved 'parties' that were implicitly revolutionary in so far as they employed existing state organs like assemblies of estates against the state. Second, they were revolutionary in composition, containing a radical wing of social groups with a relatively small stake in the existing political, economic, and social scheme. Groups represented were skilled and unskilled workers, shopkeepers, in some instances lawyers with bourgeois backgrounds, and even the lower nobility. Third, these movements had a more conservative wing that provided the basically aristocratic leadership needed for success, men like Condé, Coligny and Navarre for the Huguenots, Guise and Mayenne for the Leaguers, and Orange for the Dutch rebels.[13]

All of these points are accurate, and help us to identify the forces of violent change during the late sixteenth-century religious wars. Yet one wonders whether the movements were all that 'revolutionary'. In labelling them as radical, minority 'parties', Koenigsberger may be unconsciously identifying them with the very different modern parties of revolution such as the Jacobins and the Bolsheviks. Only in so far as the sixteenth-century parties could be pushed into developing their own organizations such as the Catholic League could they be described as openly revolutionary. Even so, as the author points out, there was a fatal division between impatient radical followers and frightened conservative leadership that could produce success only if the conservatives won out—in which case they would not make basic changes in either government or society.[14]

When Koenigsberger turns to the seventeenth century, he is confronted by the same self-defeating social polarization among the rebels. This undercuts his sweeping affirmation that there was a 'genuine crisis of societies and of their political constitutions'. If there were challenges to the 'forms of patrician government' and the 'existing structure of society', as he alleges, such challenges could come only from the impotent, relatively poor. Koenigsberger does not convince the reader by stressing popular movements like the Ormée at Bordeaux, since it came after the main elements of the Fronde had already been subdued. More significant is the admission by the author himself that the parliamentary and noble leadership of the Fronde 'took great care not to commit

themselves, or to be committed to social revolution, as had happened sixty years earlier with the Holy League of Paris'.[15] Popular revolts at Bordeaux, Naples and Palermo in the mid-seventeenth century look at best like pale reflections of the massive but unsuccessful popular riots of 1566 and 1577 at Antwerp, Brussels, Ghent and elsewhere against Church and state. If popular radicalism, an alienated élite, and revolutionary parties could constitute revolution in later times, they were not revolutionary movements, successful or abortive, in the sixteenth and seventeenth centuries. To borrow the terminology of social scientists, they were ingredients of rebelliousness and rebellion against the personnel and part of the authority, but not the basic structure, of regimes.

The subject of rebellious elements leads us directly to the second major question raised by the Forster-Greene and Koenigsberger volumes, namely, what would make revolution rather than mere rebellion possible? Or, to use the language of the social sciences adopted by Forster and Greene, what were the underlying 'preconditions' as opposed to the short-run 'precipitants' and 'triggers' of revolution? The best starting place for such a discussion is, paradoxically, Koenigsberger's treatment. The very fact that the factors he sees lying behind Early Modern upheaval are really preconditions of revolt, rather than preconditions of revolution, is immensely helpful here. Once we can identify the factors underlying rebellion, we can seek the other missing ingredients necessary for a true revolution to break out.

Koenigsberger sees the mid-sixteenth- and mid-seventeenth-century crises as the breakdown of a very tenuous arrangement of Emperor Charles V's time between rulers and upper social orders. That agreement had thrown the two parties together in fear of popular movements like the Spanish Comuneros. It had made it possible for the upper echelons of society and state governments to usher in early Reformation changes, without revolution, through existing state structures. Even during the period of accommodation and co-operation, however, monarchy was unhappy with the fiscal exemptions and other concessions granted to nobles and some wealthy bourgeois. In turn, those socially superior elements were restive with their place under monarchy, while a third group, the poorer sort of people, were unhappy with their inferior position beneath both Crown and upper social orders.

Though Koenigsberger does not separate what social scientists would call triggers and precipitants from underlying preconditions of upheaval, it seems clear that he would see as the basic causes of revolt the following. A regime that was never really strong clashed with powerful social elements that were disenchanted with their political role, during periods of war and fiscal crisis (triggered by a banking crisis in the 1560s and by depression from the 1620s to the 1640s). More specifically, these military-fiscal conditions not only exasperated the rich and the poor through new tax burdens, but also demonstrated by the weaknesses of the state's fiscal-bureaucratic machinery that central governments could not enforce their absolutism.[16] The role of religion from the 1560s to the early stage of the Thirty Years' War is not as clear, but

whether one could call it a precondition or a trigger, it served to inflame passions against states and temporarily to unite the élite and poorer elements of protest. However, religion failed to fuse these rebel elements into a viable, basic alternative to the existing regimes. Instead, counter- or anti-revolutionary social divisions between rich and poor sapped the strength of ideological religious forces that were working in conjunction with a weak regime and antagonized subjects towards revolutionary change.[17]

It is interesting that these socio-economic factors that held back the 'revolutionary parties' described by Koenigsberger constitute the weakest link in the chain of Forster and Greene's revolutionary preconditions. Obviously the editors were trying to compress, but the vagueness, and inconsistencies in their treatment of the precondition of 'fierce social antagonisms' betray more fundamental weaknesses. To be sure, they perceive that Early Modern social and material discontents would not, of themselves, have caused any of the potentially or actually revolutionary uprisings of the times. They probe still farther in stating that severe economic hardship and hostility to social superiors by the lower orders in Sicily and Naples simply could not sustain rebellion, let alone lead it to success. Yet they suggest mysteriously that 'powerful socio-economic sources of discontent may have been necessary to transform a revolt or a rebellion into a revolution', citing the Dutch and English 'revolutions' as examples. The reader cannot help becoming critical of such unresolved differences, especially when Forster and Greene place together these specific nonrevolutionary and general revolutionary ingredients under the rubric of 'fierce social antagonisms', or, to borrow the language of social scientists once more, 'multiple social dysfunction'. Such categorizing is not a perceptible improvement on Koenigsberger's pragmatic treatment of social conditions. There is one sentence in the Forster-Greene introduction that sums up the frustrations they must have faced in grappling with socioeconomic factors, though it also gives us a glimpse of other, more intelligible, preconditions which those editors discuss clearly without becoming entangled by terminology. To quote the full sentence:

> The most politically dangerous social antagonisms in early modern Europe thus seem to have derived not from class hostility but from status inconsistencies arising not only out of changing economic conditions and frustrated material expectations but also, and even more important, from government actions which directly threatened the *situations acquises* and the local power of traditional elite groups ranging from municipal councilmen to high nobles.[18]

The glimpse of better things to come in the previous quotation refers in part to revolutionary preconditions that Koenigsberger would accept as important, notably the 'defection of some of the elite', and the 'crisis of regime', which is subdivided by Forster and Greene into a regime's structural weaknesses and its aggressive attempt to cover up by asserting authority. But in addition, the editors

develop ideological factors that are missing from Koenigsberger's account. Particularly impressive is their use of the term *patria*, which Professor John H.Elliott has brought to the attention of Early Modern scholars. The *patria* provided an alternative focus of loyalty competing with the State, not in the sense of modern nationalism but the rallying by a local or national community and its leading elements in support of its established constitutional system. The *patria* was a precondition of rebellion in the sense that it could produce spontaneous resistance to any outsiders or other persons who dared to tamper with the *status quo*.[19] Despite the backward-looking nature of this rebelliousness, Forster and Greene can view it as a potential precondition of revolution. As they point out, such appeals to tradition could be turned into forward-looking 'demands for basic alterations in the political system or constitutional structure'. Without developing this theme, the editors tantalizingly speculate that 'religious divisions and a strong moral content may have been powerful catalytic agents in pushing the thought of discontented groups beyond traditional bonds'. And they conjecture further that 'a major intellectual breakthrough was possible only in large upheavals [e.g. the Dutch and English revolutions] that stimulated prolonged and intense debates about the fundamental postulates of political and social organization'.[20]

Two other preconditions appear in Forster and Greene's list. One is the existence of an institutional vehicle of protest—a House of Commons, the Catalan Diputats, Dutch States-General and provincial estates, or the Parlement of Paris. This is the Forster-Greene category that most precisely fits the meaning of precondition as opposed to an actual 'cause'. The final precondition is really a combination of the long-range emergence of an opposition and its short-term initial acts of rebellion. Specifically, there must be a feeling that success lies around the next barricade, an atmosphere that is essential for bringing wavering members of the upper social orders into open support of the rebel side.[21]

This is an impressive list of preconditions. It shows quite dramatically how far we have come from the once-celebrated analysis in Crane Brinton's *Anatomy of Revolution*. Admittedly, that trail-blazing scheme was partially predetermined by the author's decision to restrict his analysis to four 'major' revolutions, the Puritan, American, French, and Russian. In any case, Brinton's categories contained preconditions now considered too general to be of much use: economic progress, intensified hostility between social groups, the alienation of the intelligentsia, and a weak, insecure regime faced with a fiscal crisis.[22]

Some social scientists who are more experienced in the craft than Forster and Greene can go still farther in probing into revolutionary origins, weighing as well as counting preconditions, linking them to each other, and in the process developing them into highly complex, extremely plausible factors lying behind upheaval. What Brinton called the alienation of the intelligentsia, Forster and Greene the defection of the élite, and Koenigsberger the intensified opposition of old privileged classes, can be utilized in surprisingly new ways. Merely by rephrasing these descriptions as hostility towards a regime by persons normally

non-criminal in behaviour, scholars are forced to ask why such people would become political criminals. Immediately, Brinton's explanation that these persons were progressing economically has to be discarded as psychologically shallow. Social scientists can then bring out a point that is implicit only in Brinton, namely that political criminality stems from the general frustration of expectations rising far beyond actual economic benefits. Moreover, this thesis can be pursued to the conclusion that the frustration occurs under differing circumstances. A prosperous person may envy a rival, idealized social group that enjoys greater economic success. A similar individual may feel bitter when caught in a short-run recession after prolonged prosperity. These factors are not even implicit in Brinton, and they are all but missing from Forster and Greene. Yet they are well known as the complex economic-psychological precondition labelled the 'J curve'.[23]

If one asks 'how' as well as 'why' seemingly prosperous establishment types can turn against a system, social scientists provide an answer by linking psychological factors with ideological and political as well as economic preconditions. There is the interesting hypothesis that a new ideology focuses an individual's attention on a single objective by destroying the delicately balanced multiple-role-playing that normally keeps him going in so many directions that his rebelliousness is stifled. The same person is thereby psychologically prepared to become bolder when the existing regime exhibits by bankruptcy or foreign defeat that it cannot and will not cope with internal frustrations and discontent. While it is fair to note that this last factor is almost identical with the Forster-Greene precondition of the feeling of imminent success, that partial explanation of revolution has been made more intelligible through its incorporation in the cohesive economic-intellectualpsychological analysis.[24]

Even the most sophisticated treatment of revolutionary preconditions by social scientists remains unconvincing for the historian who wants models discarded ultimately in favour of actual historical situations. Unfortunately, the Forster-Greene scheme does not shake itself entirely free from the hypothetical approach it has inherited from the social sciences, despite the factual buttressing of the succeeding essays on individual revolts and revolutions. We are presented with factors that could lead to revolution, but are left asking whether they actually did, since many revolutionary preconditions turn out to be the norm in early modern Europe, existing in places that did not revolt as well as in rebellious and revolutionary regions.[25] Thus the *patria* was a very widespread Early Modern concept; we need to compare its ideological importance in areas that revolted with its impact on those that remained docile. Almost as intriguing is the role of institutions that could become vehicles of protest. The one state that lacked the social estates and political provinces necessary for representative institutions to thrive was Russia; yet there has been virtually no comparison of Russian uprisings with Western ones, or—with the exception of Elliott's essay in the Forster-Greene volume —of docile and rebellious Western assemblies.

If it is retorted that Early Modern revolutions were caused by a combination of preconditions, thus rendering any comparative study of one precondition irrelevant, a second objection can be raised. The supposed preconditions that are most crucial to such a combination theory, because they occurred more often in rebellious than in peaceful states, are not really preconditions after all. Rather, they most closely resemble 'precipitants' and 'triggers', precisely the types of short-run causes that the editors Forster and Greene have warned the other contributors in their volume to avoid discussing.[26] For example, the defection of the élite, a frequent possibility throughout Europe during the Early Modern period, occurred prior to other defections in only one major upheaval, the Puritan Revolution. In Catalonia, it came after rebellion by the lower social orders, and in Portugal well after that stage. One could argue the priority of either popular revolt or élite defection in the coming of the Fronde, and the Dutch revolt.

The 'weakness of regime' concept is perhaps the chronologically weakest candidate for 'revolutionary precondition' status. Vulnerability was a universal trait of Early Modern times, becoming more and less pronounced almost in geographic and chronological cycles. Was not Christina of Sweden's regime dangerously weak right after the Thirty Years' War, being hard pressed to handle an unhappy nobility, disgruntled clergy, merchants and peasants, and a remarkably viable parliamentary tradition? Denmark was more vulnerable still, having lost its foreign wars against Austria and Sweden. Russia had perhaps the weakest of all the mid-seventeeenth-century regimes, if we take into consideration the after-shocks of the Time of Troubles, the freezing of social status and domicile by the 1649 Law Code and the impact of the Nikonian religious reforms. The point is that governmental weaknesses, élite dissatisfaction, and institutional vehicles of protest all had to be tested by political crisis.

In effect, the two basic questions regarding the nature and preconditions of revolution raised by the Forster-Greene and Koenigsberger volumes have converged with the same positive suggestion. It has become clear that we can most effectively test theories of preconditions and explore the nature of revolution in Early Modern times by studying the background of individual uprisings. For this study, the analyses of specific upheavals in the ForsterGreene volume by Professors Raeff, Mousnier, Elliott, Stone and Smit are particularly helpful.

Marc Raeff's essay on Pugachev's late eighteenth-century regional revolt against tsarist Russia, though clearly out of place in a discussion of sixteenthand seventeenth-century western European uprisings, does tell us some things. First, it indicates how indispensable the western European institutional bases of opposition were for even a mere rebellion, and especially for a revolution. Lacking the West's built-in protest vehicles of representative assemblies and royal princes, Russian rebels worked clumsily time and again on behalf of a poor substitute—the almost mythical 'true Tsar'.[27] Second, though Raeff handles skilfully the complex relations of the many rebel groups, from Cossacks to

Bashkir and monastery peasants, he fails to convince the reader that their movement had either a workable programme or hope of overthrowing the distant central regime. This underscores the generalization that those missing ideological ingredients must exist before historians can apply the term 'revolution', in either the potential or the actual sense. Raeff tries to link the Pugachev affair to Western traditions of rebellion by suggesting that both 'exemplified the discontent and rebelliousness of a traditional group in the fact of the transformations wrought (or threatened) by a centralized absolute monarchy'.[28] He only succeeds in conveying the impression that the Cossacks were closer to primitive rebels than to rebellious system-builders. Indeed, it seems reasonable to accept the social scientists' labelling of Pugachev's revolt as another jacquerie, or 'simple rebellion'. After all, it was essentially a church and king protest on behalf of existing traditional authority, aiming solely at purging state and society of bad personnel.

The ideological and institutional bases of major, violent change are much more apparent in Roland Mousnier's analysis of the pre-Fronde. His description of France's government fits the 'weakness of regime' theme in so far as its ministers were hated for fiscally harsh internal programmes and aggressive foreign policies which remained unclear to many subjects. More important still for Mousnier was the façade of constitutionalism on the side of the opposition. Combining ideological and institutional factors, he notes that many within the upper echelons of society thought the king should rule in accordance with 'royal edicts registered by the parlements and…certain habits and customs,…the so-called fundamental laws of the kingdom'. Acting on such a belief were princes who claimed a right to participate in upholding this constitution, either by advising the king or rebelling against his 'evil' ministers, the corporate bodies of officials (notably the parlements which, in their capacity as judicial guardians of equity for subjects, blocked legislation) and finally the organizational elements of single provinces which reacted against royal 'violation' of their customary tax exemptions and other privileges.[29]

Mousnier's argument falters only when he begins to contend that this opposition's attack on the actual constitution in the name of their idealized constitution created a revolutionary situation. In practical terms, their institutional vehicles were incapable of either overthrowing or taking the place of absolute monarchy. For the key opposition institution, the Parlement of Paris, antagonized rival law courts in the capital, and never spoke for the provinces outside central France. Moreover, that tribunal continually drew upon the monarchy's own principle of divine right absolutism, although it did so in a highly complex manner. Revolution was also prevented socially by divisions between the judicial and princely-noble branches of the opposition, as well as by the primarily neutralist role of the peasantry. While many urban artisans and petty bourgeois did rebel against royal authority, peasants frequently tried to fend off royalist and opposition attacks on their possessions.[30] Thus, despite strong ideological and institutional bases of revolt, plus the existence of alienated

elements within the upper social orders, some popular discontent, a somewhat discredited regime, and many socio-economic factors such as climatic adversity, frequent plagues and economic recession, these preconditions of revolt do not add up to preconditions of revolution.

Perhaps one could type the Fronde as a very 'advanced rebellion', in the Chalmers Johnson scheme. It acted on behalf of an idealized past and against existing personnel, with a few attacks on some elements of authority such as intendancies and extra-legal courts. What it did not and probably could not do was break with absolute monarchy, question the social structure, or develop an ideological alternative to existing beliefs. In short, it fell far short of being potentially or actually a 'simple revolution'.

The mid-seventeenth-century cluster of uprisings against Spain is even more illuminating than the Fronde, thanks to Professor Elliott's decision to compare all the revolts and his refusal to read more into them than the evidence warrants. He underscores Koenigsberger's finding that 'popular' revolt, lacking leadership from higher social echelons, could not be sustained for long in the sixteenth and seventeenth centuries. Indeed, he sees Early Modern Europe's hunger riots as a danger constantly threatening both the upper orders and monarchy—a perspective that contrasts sharply with Mousnier's view of the poor as largely directed by their social superiors against governments [31]

The social role of the élite in the Iberian peninsula and Spanish Italy is, instead, linked by Elliott with its institutional and ideological context. Where the upper orders had reasons to be hostile to the central regime because of a threat to their prosperity, and where they enjoyed an institutional base like a Cortes as well as a sense of belonging to a *patria*, a successful revolt could take place. That is precisely what occurred in the case of Portugal's war of independence against Spain from 1640 to 1668, though we should note with Elliott that this was not a revolution. (After all it did not abolish monarchy, it did not alter basic institutions, and it changed nothing socially.) By contrast to this successful revolt, the nobles in Sicily and Naples lacked the preconditions of Portugal's rebellion, and, in particular, were relatively satisfied with their role in government and hence more than willing to suppress rebellion by the restive native poor.

Between these two types of Hispanic experience was Catalonia, which had both the élite revolt experienced in Portugal and the popular uprisings associated with Sicily and Naples. The result was an upheaval with far greater potential for change than the Portuguese political revolt, thanks to popular, radical involvement. 'Yet, ironically, the Catalan leaders' fear of popular revolt turning against them gave the general Catalan uprising against Spain far less chance of succeeding than the Portuguese. Because of this balancing of tendencies, Elliott stops short of Forster and Greene's editorial suggestion that revolution might have come out of the breadth and depth of Catalan hostility to Spain.[32]

The Russian, French, and Hispanic cases show that institutional and ideological factors were preconditions to Early Modern rebellion, and, just as

clearly, that uncertain social conditions thwarted successful revolt or attempted revolution. With these findings in mind, it is pertinent to concentrate on the troublesome social aspect of pre-revolution within those uprisings which their particular historians claim were revolutions.

In the case of the Dutch uprising against Spanish rule, social factors would seem to be the only explanation for an otherwise baffling phenomenon. Indeed, the other rebellious ingredients stressed by Elliott and Mousnier were largely lacking in the Low Countries. There was such deep-rooted localism throughout the provinces of that region that one would never have predicted the emergence of an independent United Netherlands—*patria* might work to rebellion, but *patriae* cancelled out each other or at best went their separate ways. The most that can be said for institutional factors is that Habsburg centralizing efforts stiffened local oppositions, while the development of the relatively new but traditionalist States-General into a successful organ of revolt in the northern provinces was the result and not the cause of rebellion. Religion provided only a negative ideological force, as people of various religious views agreed only in hating the established church.

Initially, social conditions were equally unconducive to united revolt. Nevertheless, Professor J.W.Smit is able to expose the underlying rebellious and revolutionary aspects of Dutch society in a brilliant analysis that also buries the pet theories of Marxists, conservative scholars and social scientists. He contends that the economic situation gradually eroded the docility and particularism of one group after another. Briefly, a period of prosperity for ‘middle-class’ elements and lagging wages for poorer subjects gave way to a recession in the 1560s that coincided with imperious Spanish tax demands. Consequently, ‘the frustrated prosperous bourgeois of the booming towns joined the desperate declassed craftsmen and thriving or declining nobles’. In Smit’s words, ‘local riots coalesced into general revolution’.[33]

By itself, this initial coalescing of economically diverse interests against the scapegoat of the state does not explain the outcome of the rebellion that began in the 1560s. The story might have ended the way the Catalan revolt of the 1640s would. And, in fact, the same social flaws detected in sixteenthcentury rebellions by Koenigsberger appear in Smit’s account. In 1566 and again in 1576–9 waves of popular iconoclasm throughout the southern towns of the Low Countries destroyed what possibility there had been of a united Great Netherlands state by turning the nobles and patricians against the poor.

Still, Smit insists on looking beyond those débâcles to the ultimate winning of independence by the northern provinces, and speculates on the reasons for that partial revival of the rebel cause. There is the important, if obvious, point that the northern provinces’ revolt succeeded because their middling bourgeoisie were threatened neither by the nobility, which was weaker than in the south, nor by the poor, whose lack of numbers and confusion after immigration from the south may have caused their rather mysterious docility But this absence of social conflict, even if true, explains only a Portuguese-style successful fight for

independence, not a revolution. Hence Smit has to reinterpret the nature of the independent Dutch state. In general, it can be agreed that the Dutch were revolutionary by their act of abolishing (reluctantly) the monarchy. Yet how innovative was the structure that replaced it, since the new regime had, at best, a moderately unified, flexible government combining provincial and town particularism with some centralization through the States-General? In historiographical terms, Smit's version of Dutch political change lies somewhere between the weakly federated and backward-looking state depicted by hostile, pro-absolutist historians, on the one hand, and the image of a forward-looking, economically *laissez-faire* state conjured up by the nineteenth century's liberal-bourgeois historians. Politically, this peculiarly past- and present-oriented Dutch republic does not take us very far in our quest for an understanding of the Dutch 'revolution'. But just as in his initial discussion, Smit looks to the emerging state's economic and social bases to provide the answers.

In trying to fathom how so many groups could initially rise up against Spain, Smit suggests the possibility of an underlying change in the economic and social structure which might have broken up traditional patterns of life. He speculates in particular on the sort of psycho-economic-intellectual history that Pierre Chaunu's students have employed in analysing criminal behaviour. Smit returns to this broad precondition of underlying socioeconomic change when he discusses the successful northern movement. He claims, if in qualified tones, that 'the new republic became the first real capitalist and bourgeois nation with a strongly marked, very mercantile national identity'. Just what the bourgeois 'mentality' and 'consciousness' were is precisely what he says we need to explore.[34] But at least Smit's emphasis on the changing role and nature of the rebellious Dutch élite marks an advance on the revolutionary precondition of the mere alienation of a traditional and entrenched élite. As we shall see, Lawrence Stone's account of the Puritan Revolution demonstrates that Smit's hypothesis is worthy of serious consideration.

Professor Stone has the best assignment of the Forster-Greene volume in explaining the one upheaval most historians agree was a true revolution, and he does full justice to his subject. He refuses to become entrapped in historiographical arguments, methodological quibbles, or the morass of revolutionary events themselves. And, ideally for the issues raised in this review essay, his examination of the long-range preconditions brings out the social as well as the ideological and institutional background of revolution.[35]

The most obvious precondition of the Puritan Revolution was the existing regime's ineptness. Yet the way Stone develops that theme helps to show how the government not only triggered rebellion but unwittingly prepared the way for a revolutionary onslaught that temporarily swept away Crown, Church and Lords, that is the institutional aspects of political, religious and social structure. As in the case of the Portuguese, Catalan and Frondeur uprisings, there was a heavy-handed centralizing movement by the monarchy that acted as a precondition of upheaval. But in England the complementary precondition of the

regime's loss of inner vitality had also been at work, a selfdestructiveness that can be traced back from the regime of Buckingham in the 1620s to Henrician-Elizabethan policies. That weakness was so fundamental that, when revolt came against Charles I's personnel and authority, it quickly became revolution against the structure of the regime as well.

Stone is particularly convincing in probing the weakness of the Tudors' religious settlement, a masterful political step that staved off continentalstyle religious strife only at the terrible price of manufacturing a compromise church from which few subjects could receive spiritual nourishment. The same clever, but ultimately disastrous, political negativism is evident in the fiscal, military and courtly areas. In all three spheres, Elizabeth and the Stuarts were too wary of antagonizing powerful subjects to gamble on centralizing measures that would have provided greater fiscal strength as well as a broader base of social support.[36]

Stone's analysis of the opposition's institutional, ideological and social bases goes still farther than the 'weakness of regime' in explaining how rebelliousness could become revolution. Institutionally, there was of course a single Parliament for the entire realm. In the author's words, it was 'a powerful national representative body',[37] though one could qualify the point by arguing that its power derived less from medieval roots than from its revitalization as the agent of Reformation legislation. There was certainly a contrast with the other great national assembly of the period, the French Estates-General, which eventually got on the losing side of the religious wars after being the pathetic plaything of court factions. No other assembly in a state that rebelled was truly national. Both the short-lived States-General of the entire Low Countries and its successor in the northern, Dutch, provinces represented regional assemblies and corporate interests rather than the upper socio-economic orders throughout the realm as in England.

Stone's arguments about the ideological foundations of the opposition are more debatable than his convincing picture of an ideologically weak regime. Even so, his general emphasis on early modern sceptical ideas, country virtue versus courtly vice, and puritanism in the broadest sense is well placed.[38] The difficulty lies mainly in his insistence on seeing the English situation in a vacuum. If English sceptical ideas undermined support of the Stuart regime, how do we account for the fact that leading French intellectuals such as Montaigne, Pascal, Descartes and Bayle used scepticism and intellectual doubt as an argument on behalf of absolute monarchy?[39] And in contrast to the revolutionary outcome in England of the ideological struggle by the virtuous 'country' against courtly vices, a strongly imbedded 'country' reformist ideology in Philip III's and Philip IV's Spain failed to merge with rebel political and social opposition.[40] Pre-revolutionary legalism in England needs also to be tested in a comparative context, though Stone's assumption of its uniqueness will probably stand up to close scrutiny Even the pre-Fronde legalism of the parliamentary judges was too closely associated with absolutism, perhaps because of the lack of a French

common law tradition, to provide the sort of alternative ideology for outright revolution that English law did.

Stone's best case for ideological preconditions of the English revolution of the 1640s rests with the 'puritan' mood, and he opts for the broadest definition of puritanism—'a desire…to moralize Church, society, and state'.[41] This drive to purify and purge, together with the self-confidence, organization and leadership of the Puritans in the stricter, religious, sense, contrasts sufficiently with Caroline courtly attitudes to justify the old term, Puritan Revolution. Yet, as Stone says, the consequences of Puritanism are plainer than its very obscure causes. We still cannot explain why there were so many mystical conversions in the decades before the revolution, including some among parliamentary leaders.[42] Nor is there a completely satisfactory explanation for the quite different behaviour of those French Catholic 'Puritans' known as Jansenists. Potential leaders of political rebellion like Saint Cyran underwent religious conversion, only to take up a fatalistic, almost otherworldly, stand.[43] The failure of the conservative but religiously reformist Devout movement and the ultimate collapse of the zealously moralistic Company of the Holy Sacrament in France are equally puzzling. Together, these negative French counterparts of dynamic English religious currents point to a conclusion that is obvious, significant, and virtually ignored in Early Modern historical studies: the country most approaching England in ideological roots of revolution nevertheless lacked the intellectual climate necessary for the growth of a radical political opposition.

We are thus left with the rather bald generalization that ideology was a key, and may be the key to the institutional changes of the Puritan upheaval. It may well have been the motor, the driving force of the opposition. Yet it was not an all-powerful precondition of revolution. It could not fuse the antiabsolutism of the upper echelons of society with any radical rebelliousness by their social inferiors, any more than religion could in the Dutch revolt. Nor could it turn political revolution into any kind of social revolution. In both respects, seventeenth-century English ideology of the opposition fell far short of the work of the pre-1789 Enlightenment.

The socio-economic aspects of the English opposition are equally interesting, particularly since Stone avoids the upper- versus lower-class dichotomy so evident in most accounts of Early Modern rebelliousness. Instead, he contends that men of the middling sort swelled the ranks of the discontented. Behind this move, the author detects a massive shift of relative wealth from the very rich and the poor towards the 'upper middle and middle classes'. Thus friction increased between the traditional wielders of power—the Crown, courtiers, higher clergy, the aristocracy—and the growing forces of gentry, lawyers, merchants, yeomen and small tradesmen. Hostility intensified when the monarchy failed to assimilate into the ranks of the old socio-political élite the aggressive elements of the middling sort whose political power did not match their increase in wealth or their social aspirations.[44]

Can it be that Stone has hit upon one of the missing revolutionary links in most Early Modern revolts by stressing the restructuring of élites? On the surface, it is a more plausible precondition of revolution than either the mere alienation of an existing socio-political élite (that could lead to rebellion at the very most) or the currently popular historical view of a split right down the ranks of an élite (which could tear apart an Early Modern state in an inconclusive civil war). Moreover, from our study of the so-called bourgeois role in the Coming of 1789 we have evidence that a basic changing of the balance among élites and their values can be crucial to the shift from rebellion to revolution, from political to social revolution, and from hope of success to sweeping triumph. Despite Professor Smit's failure to prove the existence of such a socio-economic metamorphosis in the breakaway northern Netherlands, he is also on the right track in suggesting a partial reordering of society as a precondition of Dutch revolution. And the very fact that the Dutch were not as revolutionary as the English can be attributed to the lack of an emotional target which could increase their emerging élite's self-consciousness. To be more specific, the Dutch, in contrast to the English, could not vent their rage on a sizeable court aristocracy.

In England, as in the northern Netherlands, one other social factor made it possible for aspiring segments of the well-to-do to revolt. This was the relative absence of widespread agitation by the lower orders. The Dutch rebels did not need to compromise with Spain in order to keep their own social inferiors in check. The English rebels made their peace with monarchy only after the death of their leader, Oliver Cromwell. However, it is a mystery how the English and Dutch joined the Portuguese in escaping the contemporary European onslaught of popular discontent, which was as marked in Russia as in Catalonia, in Sweden as in Sicily.

Thanks to the specialists' analyses of particular Early Modern revolts, the broad interpretive essays by Koenigsberger, and the suggestions for organizing revolutionary studies made by Forster and Greene as well as by social scientists, we have arrived at a broader view of Early Modern upheaval than any one scholar could have provided. To be sure, this new perspective is merely a halting preliminary to what can be done in the field of comparative Early Modern rebelliousness. For that reason some of the ground already covered deserves to be recrossed in a brief recapitulation.

First, we have isolated several preconditions of revolt in Early Modern Europe: a discredited regime; alienated elements of the upper orders comprising a socio-economic élite; economic and political stresses that threatened their position; some vehicle of protest; and perhaps the concept of a *patria*. Once those preconditions existed, the injection of short-term triggers could stampede rebel elements into rallying together and either altering some personnel and policies, as the Portuguese rebels did, or forcing the central regime to restore local or group privileges, as occurred in Catalonia.

In the second place, additional preconditions were needed to lead to a revolution in the sense of an attack not just on governmental personnel, policies

or authority but against fundamentals like the regime or type of governmental structure, the existing socioeconomic élite, and the very way in which social and economic structure shaped and upheld its upper orders. Political revolutions against governmental structure were possible in England and the Netherlands because the existing regimes were not only aggressive and disliked, but to a greater or lesser degree lacking in inner vitality. In the English case, this self-destructive tendency existed in virtually every sphere of governmental activity. In the Dutch case, it was revealed in at least two ways —the thorough unpopularity of the state church, and the Habsburgs' failure to develop unifying and dynamic organs of central government for the entire Low Countries.

Quite clearly, too, something had to happen in the socio-economic sphere before either a 'simple' political revolution or a 'great national' social and political revolution could erupt. Even a political revolution had to have more than the support and leadership of elements from the existing élite, whether this came from nobles, high office-holders or well-to-do commoners. Some change in the nature of the socio-economic élite was essential, as Smit suggests tantalizingly, for the 'bourgeois-capitalist' Dutch revolution, and Stone argues more elaborately and convincingly for the revolution of the middling sort in England.

Great difficulties remain in showing how such a transformation of élites could get under way. Here we can draw negatively on the findings of Koenigsberger, Elliott and Smit, our knowledge of positive social factors at the beginning of the French Revolution of 1789, and several ideological factors.

To take negative clues first, it is evident that during the sixteenth and seventeenth centuries anything more than a minimal, docile role by the lower orders could immediately dampen or extinguish the flames of an élite rebellion. Yet some sort of terrorizing by the poorer sort was, paradoxically, a major factor in radicalizing élite movements. Even the popular 'furies' during the early stages of the Dutch revolt loosened bonds with the state as well as retightening them. It has also been argued that the Puritan Revolution was accompanied by just enough agitation from below to force the upper orders more to the left than they were prepared to go.[45]

Even more radicalizing was to be the effect of peasant jacqueries on France's so-called bourgeois revolution of 1789. Yet we cannot help asking how these French-style popular furies which had terrified upper-order rebels in the religious wars and frightened them during the Fronde, could work such revolutionary magic a few generations later. The answer lies partly in the shifting position of the French bourgeoisie between 1648 and 1789, a shift that was largely determined by bourgeois hostility to growing co-operation between the older, noble élite and the monarchical regime. An anticipation of that situation can be found in England prior to its Puritan Revolution. The existence of an English aristocracy more prominent than the Dutch one and having elements closely identified with the Stuart court helps to explain a political revolution greater than the Dutch one, and a social revolution (against the House of Lords) that approached the intensity of the initial stages of the French Revolution. Of course

in 1789, the aristocratic-royal alliance, being much stronger than the English one of the 1620s and 1630s, drove the French 'bourgeoisie' not only to attack monarchy but towards a new social concept of élitism based on talent and wealth instead of birth and status.[46]

Some of the mystery surrounding changing élitism can perhaps be dispelled by social psychologists. They already have a convenient scheme in the 'J curve' economic theory. Hence it should be easy to add to the psychological and other factors now encompassed in that explanation—the coming of prosperity, subsequent new élite power, followed by short-run recession or fear of the loss of benefits, frustration caused by rivalry with a previously established élite, and sudden, almost unresolvable, political problems.

Yet even if such a complex of factors makes a revolutionary act by well-to do persons intelligible, some sort of long-term ideological preparation seems to be necessary to explain fully the outbreak of a revolutionary psychology. There had to be some generalized moral and intellectual concepts undergirding grievances, dissolving habits of obedience and articulating new aspirations. The Enlightenment provided these, giving the late eighteenth century what the previous two centuries had lacked, something that could not be provided by any brand of Christian belief, any revolutionary 'party', any concept of a *patria*, any charismatic leader, or even a millennial futuristic or idealized traditional goal. In a sense, this means that full-blown revolution was not really possible before the late eighteenth century, that is not until people looked to the future rather than to the past for hope, to earth and not to heaven, to written constitutions, not to eternal, God-given natural law. In another sense, revolution is a modern phenomenon; as stated at the outset, there has to be a regime sufficiently centralized to be overthrown and replaced, not just deprived of a province or a prime minister.

Ironically, too, revolution was possible by the eighteenth century precisely because the political stability that accompanied centralization made men think revolution would not erupt. If Forster and Greene are correct in saying that revolts occur because would-be rebels think they can realize their aims, the fact remains that Early Modern peoples, in general, dreaded the effects of a major upheaval. It is no coincidence that the most profound upheaval of the sixteenth and seventeenth centuries occurred in England. As Lawrence Stone so astutely notes, fears of a succession dispute turning into civil war, of uprisings by poor against rich, and of foreign invasion had died down between the mid-sixteenth and the mid-seventeenth century; it proved ironically fateful for Charles I that he was the first king since Henry VIII with a fully undisputed title.[47] The contrast between the English revolutionaries and the French Frondeurs, who tried to avoid any comparison with the Leaguers of the French Wars of Religion and were horrified by the judicial murder of Charles I, cannot be stressed too much.[48]

Late in the seventeenth century John Locke added important ideological underpinnings to this English 'revolution-without-fear' mentality. Picking up the loose ends of a double contract theory of government that several continentalists

were developing, Locke implied that rebellion in the sense of removal of personnel (legislative or executive) would not cause serious turmoil. Rebellion would dissolve the political contract between society and governors, only to leave intact the prior social contract which had established a stable society and made possible the second, political agreement.[49] It would be interesting to pursue the development of that mental and ideological sense of security in eighteenth-century continental circles and within the thirteen of the British American colonies that ultimately engineered the American Revolution.

The other side of this ideological coin shows the absence of that secure feeling during most of the Early Modern period. It is an anti-revolutionary precondition we historians should not ignore. This does not mean that we should write off the word 'revolution' as irrelevant to historical studies of Europe before the late eighteenth century, for sometimes backward-looking movements, aided by an unusual coalescing of factors, can become revolutionary. The English and Dutch revolutions were, indeed, partly the product of backward-looking, confused peoples thrust in the midst of a muddled political situation, though those upheavals also anticipated aspects of the forward-looking preconditions lying behind late eighteenth-century revolutions. What we should try to avoid is the glib use of question-begging labels like 'revolution', or that even more honoured term of our time, 'crisis'.[50] With these qualifications, we can still take advantage of the admittedly rigid models of types of upheaval and categories of revolutionary and rebellious preconditions. Both devices can open up questions and develop answers about upheavals between 1560 and 1660 that have been obscured too long by lack of historical imagination and daring.

NOTES

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1 H.R.Trevor-Roper, 'The general crisis of the seventeenth century', *Past and Present*, XVI (1959), pp. 31–64. The ways in which this 'crisis' thesis has been superimposed on traditional textbook accounts vary. Compare J.Blum, R.Cameron and T.G.Barnes, *The Emergence of the European World* (Boston and Toronto, 1966), Chapter 9, The age of crisis, 1600–1660: absolutist solution', especially the Introduction, pp. 206–7; J.R. Strayer, H.W.Gatzke, E.H.Harbison and E.L.Dunbaugh, *Western Civilization since 1500* (New York, 1969), Chapter 4, 'Political and economic crises of the seventeenth century', especially p. 111; and the more sophisticated J.R.Major, *Civilization in the Western World: Renaissance to 1815* (Philadelphia and New York, 1966), part 4, 'The seventeenth-century crisis, c. 1560–1715', especially pp. 232–5, 282–4. For criticisms of Trevor-Roper's 'crisis' see especially *Past and Present,* XVIII (1960), pp. 8–42, notably the comments by R.Mousnier, who contrasts France and England, and who had earlier developed the crisis theme in his *Les XVIe et XVIIe Siècles* (1st edn, Paris, 1954) constituting vol. IV of M.Crouzet, ed.; *Histoire générale des civilisations* by J.H. Elliott, who questions the applicability of the English type of revolutionary

crisis to Europe; and by E.H.Kossmann, who does not believe there was a midseventeenth-century crisis. The most constructive use of the Trevor-Roper thesis has been by M.Roberts, 'Queen Christina and the general crisis of the seventeenth century', *Past and Present*, XXII (1962), pp. 36–59. The published version of a conference on 'Seventeenth Century Revolutions', *Past and Present*, XIII (1958), pp. 63–72, shows what specialists can do when confronted with the opportunity to compare several revolts.

2 The most helpful treatment for me has been by Chalmers Johnson, *Revolutionary Change* (Boston, 1966). All historians will enjoy and profit from the critique of the work of social scientists by Lawrence Stone, 'Theories of revolution', *World Politics*, XVIII (1966), pp. 159–76. See also the wide-ranging essay by P.Zagorin, 'Prolegomena to the Comparative History of Revolution in Early Modern Europe', *Comparative Studies in Society and History*, XVIII (1976), pp. 151–74.

3 H.G.Koenigsberger, *The Habsburgs and Europe 1516–1660* (Ithaca and London, 1971), pp.xv, 304. The three chapters were originally published separately in volumes II and III of the *New Cambridge Modern History* (1958, 1968), and inH. R.Trevor-Roper (ed.), *The Age of Expansion* (London, 1968). The main theme of the essays concerns interstate relations: the Spanish Habsburgs' imperial mission shifted from Charles V's ideal of universal Christian empire to Philip II's less grandiose groping towards a Spanish-Catholic imperialism on behalf of CounterReformation, and culminated during the Thirty Years' War in the collapse of the still more restricted vision of a Castile-dominated Spain playing a 'CatholicChristian and politically pre-eminent role in Europe and the world'. Despite the provocativeness of that interpretation of Early Modern imperialism, and the equally striking conclusion that Westphalia replaced Charles V's 'old, imperialist ideal' with the 'idea of a universal guarantee of peace', the author's treatment of foreign affairs is not as conducive to debate and innovative work among historians as sketchier, but bolder allusions to internal affairs and revolution in particular, at various points in all three chapters.

4 Robert Forster and Jack P.Greene (eds), *Preconditions of Revolution in Early Modern Europe* (Baltimore and London, 1970), p. 214. The book was the product of a colloquium on modern revolutions by the editors, a series of talks by experts of particular Early Modern uprisings, and commentaries, involving J.W. Smit and Herbert H.Rowen, Lawrence Stone and Perez Zagorin, J.H.Elliott and Ruth Pike, Roland Mousnier and Orest Ranum, and Marc Raeff and Michael Cherniavsky, all undertaken under the auspices of the Department of History at the Johns Hopkins University in 1968–9.

5 Koenigsberger, *The Habsburgs and Europe*, pp. 64, 217, 276, 283.

6 Forster and Greene, *Preconditions of Revolution*, p. 1, quoting Eugene Kamenka, The concept of a political revolution', in Carl J.Friedrich (ed.), *Revolution* (New York, 1966), p. 124.

7 Johnson, *Revolutionary Change*, Chapter 7.

8 Forster and Greene, *Preconditions of Revolution*, p. 12.

9 G.Pettee, 'Revolution—typology and process', in Friedrich, *Revolution*, pp. 10–33.

10 J.Rosenau (ed.), *International Aspects of Civil Strife* (Princeton, NJ, 1964). I prefer Roseneau's terms to the more ambiguous 'government', 'regime', and 'society' or 'community' in Chalmers Johnson's similar scheme. The body-counting quantitative approach is exemplified by R.Tanter and M.Midlarsky, 'A theory of

revolution', *Journal of Conflict Resolution,* XI (1967), 264–79. This approach creeps into Stone's essay in the Forster-Greene volume, where he notes the wordiness of the English Revolution (22,000 pamphlets and newspapers in twenty years).

11 This criticism is based on Johnson's *Revolutionary Change,* Chapter 7, which has lost as well as gained some things in reducing the six types of violent upheaval in his earlier work, *Revolution and the Social System* (Stanford, CT, 1964). Stone's favourable evaluation is based on the earlier six-part scheme. See his Theories of revolution', p. 162.

12 Forster and Greene, *Preconditions of Revolution,* p. 12. The dustjacket, incidentally, omits the Dutch uprising from the only truly revolutionary category, calling the English Revolution 'the one great national revolution of the period'.

13 Koenigsberger, *The Habsburgs and Europe,* Chapter 2, especially pp. 64, 196.

14 Ibid. p. 197. The author's idea of 'revolutionary parties' goes back to his justly celebrated essay, 'The organization of revolutionary parties in France and the Netherlands during the sixteenth century', originally published in the *Journal of Modern History,* XXVII (1955), and now reprinted along with a host of other interesting Koenigsberger essays in his *Estates and Revolutions: Essays in Early Modern History* (Ithaca, 1971).

15 Koenigsberger, *The Habsburgs and Europe,* pp. 281, 283.

16 Ibid. pp. 35–9, 43, 48–9, 51–3, 64–7, 279. I find it difficult to take seriously the author's emphasis on the weakness of female rule during the 1560s in a 'society whose ethos was masculine and military'. Elizabeth of England survived, while Mary Queen of Scots, Margaret of Parma in the Netherlands, and Catherine de Medici in France failed because of many personal and circumstantial factors. See p. 66 of his work.

17 Ibid. pp. 64, 197, 223–35.

18 Forster and Greene, *Preconditions of Revolution,* pp. 13–14, contains the discussion of the socio-economic precondition as well as the long quotation.

19 Elliott, 'Revolution and continuity', pp. 110–33 above; Forster and Greene, *Preconditions of Revolution,* pp. 15–16.

20 Ibid. p. 16.

21 Ibid pp. 16–17.

22 Crane Brinton, *The Anatomy of Revolution* (1st edn 1938, rev. edn 1952), Chapter 2.

23 See James C.Davies, 'Toward a theory of revolution', *American Sociological Review,* XXVII (1962), pp. 5–13.

24 These themes are particularly well developed throughout Johnson, *Revolutionary Change*.

25 Taking into account the title of the Forster-Greene book and various statements in the introduction, it is also not clear whether the editors are distinguishing between 'preconditions of revolution', 'preconditions for political upheaval… and the character of the five early modern revolutions or potentially revolutionary uprisings' and 'preconditions of political and social disturbance'. The dustjacket does clarify somewhat by stating that 'this volume examines the preconditions—or underlying causes—of eight of these [Early Modern] upheavals in an attempt to throw additional light upon the origins of the now familiar phenomenon of *revolution*'. Unfortunately the dustjacket ends by broadening this statement with

the assurance that the individual essays on the 'long-range causes of each of these disturbances' allow the editors to 'register some general observations about the nature and range of the preconditions and character of revolutions and revolts in early modern Europe'.

26 See Forster and Greene, *Preconditions of Revolution,* p. 2.

27 The Russian difficulties bear some resemblance to the traditional Chinese problem of questioning political legitimacy. Cf. the review essay on Roland Mousnier, *Fureurs Paysannes: les Paysans dans les Révoltes du XVIIe Siècle (France, Russie, Chine)* (Paris, 1967), by M.O.Gately, A.L.Moote and J.E.Wills, Jr, 'Seventeenth-century peasant "Furies": some problems of comparative history', *Past and Present,* LI (1971), p. 67. The striking contrast between such ideological contexts as the Russian Communist glorification of past revolts, successful or not, as a norm and the traditional Chinese situation that did not even have a term for revolution except in reference to legitimacy, i.e., the 'removal' of the Mandate of Heaven, does raise the basic question of whether order or upheaval is the norm in a given society's value system. Here is a fundamental question that historians, sociologists, political scientists and psychologists have barely begun to raise. J.W. Smit's opening remarks about the theory that sees revolution as 'internal war', constituting a breakdown of the peaceful, normal political process rather than an intensification of it, is sufficiently devastating to scare any modish scholar from trying to get away with a superficial exploration of the issue. See Forster and Greene, *Preconditions of Revolution,* pp. 22–3.

28 Forster and Greene, *Preconditions of Revolution,* p. 161.

29 Ibid. pp. 138–9, 142–3, 149–50.

30 In general the reader can contrast Mousnier's views, in Forster and Greene, *Preconditions of Revolution,* pp. 149, 156–7; and A.L. Moote's *The Revolt of the Judges: the Parlement of Paris and the Fronde, 1643–1652* (Princeton, NJ, 1971). There are, nevertheless, striking modifications by Mousnier of some of his long-standing interpretations, stressing the carelessness of the monarchy's articulation of its policies, the weakness as opposed to the strengths of royal manipulation of sale of offices to control parlementarians and other *officiers,* and the degree of popular revolt that was not manipulated by the upper social orders.

31 Compare Forster and Greene, *Preconditions of Revolution,* pp. 110–11 (Elliott), and p. 157 (Mousnier).

32 Compare Forster and Greene, *Preconditions of Revolution,* p. 9 (Forster and Greene), and p. 110 where Elliott states categorically that 'To search for the "preconditions" of revolution in the Spanish Monarchy, then, is to search for something rather grander than the revolts themselves appear to warrant.'

33 Forster and Greene, *Preconditions of Revolution,* p. 41.

34 Ibid. pp. 41, 43, 51–3.

35 Of course, some of Stone's present themes are elaborations on his major social study, *The Crisis of the Aristocracy, 1558–1641* (Oxford, 1965; abridged edn, 1967), just as J.W.Smit has built his own contribution partly on his earlier, sparkling historiographical essay, The present position of studies regarding the revolt of the Netherlands', in J.S.Bromley and E.H. Kossmann (eds), *Britain and the Netherlands,* I (London, 1960). J.H. Elliott's basic work behind the present contribution is well known: *The Revolt of the Catalans* (Cambridge, 1963).

36 Forster and Greene, *Preconditions of Revolution,* pp. 78–86, 95–6.

37 Ibid. p. 73.
38 Ibid. pp. 86–93.
39 See the doctoral dissertation by Dale Daily, 'Early Modern Scepticism on Political Society: Montaign, Pascal, Bayle and Hume' (University of Southern California, 1974).
40 This fact emerges in the unpublished doctoral dissertation by Theodore G. Corbett, 'The elements of reform in early seventeenth-century Spain' (University of Southern California, 1970).
41 Forster and Greene, *Preconditions of Revolution*, p. 89.
42 This fact was first brought to my attention by Professor Paul Christianson of Queen's University, Canada.
43 The sociopolitical study by L.Goldmann, *Le Dieu caché: Etude sur la vision tragique dans les Pensées de Pascal et dans le théâtre de Racine* (Paris, 1955), is fascinating, though the author's explanations of that 'tragic vision' are not entirely convincing. Dorothy Backer is now probing the psychology of Jansenist*précieuse* connections. See her *Precious Women* (New York, 1974).
44 Forster and Greene, *Preconditions of Revolution*, pp. 62–5, 94–5.
45 Brian Manning, 'The nobles, the people, and the constitution', *Past and Present*, IX (1956), pp. 42–64.
46 Several scholars are now working on this crucial question concerning 1789, including Marcel Reinhard in France, George V.Taylor in the United States and Colin Lucas in England.
47 Forster and Greene, *Preconditions of Revolution*, pp. 76–8.
48 See P.Knachel, *England and the Fronde: the Impact of the English Civil War on France* (Ithaca, 1967), an interesting genre of comparative rebellion studies that should stimulate further work of its kind.
49 Cf. A.L Moote's *The Seventeenth Century: Europe in Ferment* (Lexington, Mass., 1970), Chapter 15, especially p. 391. I would accept Chalmers Johnson's suggestion that Locke was describing a conservative rebellion designed to preserve the existing social structure by removing offending personnel. However, one must add that the effect of Locke's *Second Treatise of Government* was to make rebellion respectable, and hence to make revolution possible. See Johnson, *Revolutionary Change*, p. 136.
50 Two important essays on that term have appeared: R.Starn, 'Historians and "crisis"', *Past and Present*, LII (1971), pp. 3–22, which warns us of that word's double threat by creating vagueness in place of the precision of 'revolt' as well as by diagnosing short-term, surface sickness out of the context of long-term healthy continuity of a state or society; and N.Steensgaard, 'The seventeenth-century crisis', Chapter 2 in this volume, which, in the process of taking on the Marxist left, Mousnier right and Trevor-Roper in the middle, suggests that we play down the idea of crisis impinging on the hapless state, and instead concentrate on the state itself as neither the captive of classes nor the victim of wartime pressures but a living being playing out the role of the persons who staffed it, in the social and economic realms of the individual country.

Since the initial publication of the present article, an invaluable study of seventeenth-century France has appeared: P.J.Coveney, ed., *France in Crisis, 1620–1675* (Totawa, NJ, 1977), a unique collection of essays by Mousnier, Porshnev, and

others, with a sophisticated introductory essay on the historiography of the ‘crisis’ (by Coveney, pp. 1–63).

7
BETWEEN THE SIXTEENTH AND SEVENTEENTH CENTURIES
The economic crisis of 1619–22*

Ruggiero Romano

A few words are necessary to explain the title of this chapter. Why does it include the words ‘between the sixteenth and seventeenth centuries’ when the discussion is to be about the economic crisis of the years 1619–22? It is obvious that historical periods do not necessarily tally with the partitions of centuries: the life of a sovereign, the term of office of a minister, or a war, cannot be accommodated in the neat divisions of the calendar. The same applies to price tendencies and commercial affairs. Economic history has its own chronology A comparatively new science, it requires a periodization of its own. For the European economy, the sixteenth century ends in 1640 (although some would maintain that it ends in 1630). It is not just a matter of taking ten or twenty years from the seventeenth century and attributing them to the sixteenth, since this would be a rather pointless exercise and of little intellectual reward. My aim is, instead, to show how the crisis of 1619–22 did not merely represent a break between the centuries, but determined the character of the new century, marked the failure of the ambitions of a long period in history and, finally, was the point at which the great hopes of capitalism (obviously a mercantilist capitalism) in the sixteenth century were shattered.

In this way, the displacement of a group of ten (or twenty) years acquires a precise significance, determining the birth of the seventeenth century and engendering the spirit and characteristics that were to accompany it throughout its duration.[1]

The chapter has a further aim. Studying the internal workings of an economy in the process of development, it is possible to see the causes of recession. But it is worthwhile taking into consideration a problem known to economic historians: that while structural movements, circumstances (of long and short duration) and recessions seem to be well defined, it is in fact through a random combination of factors and their mutual interaction that economic history attains its substance and its concrete significance.

In historiographical terms, what is the sixteenth century and what is the seventeenth century? The scholar has a plethora of definitions at his disposal. To characterize the first, there is: the century of ‘price revolutions’, of ‘American silver’, of ‘modernism’, of ‘early capitalism’, of ‘economic expansion’— the list could be continued indefinitely. For the second, however, the definitions can all

be contracted to a single phrase: the century of 'economic stagnation'. I am convinced that this reasoning depends to a large degree on the history of prices. A truism has been created that a price rise equals economic expansion, while a stagnation in prices equals economic contraction. I mentioned a truism: if it were only a question of a truism, it would not matter. But the fact remains that this identity is often, too often, cited as an explanation. If the results of modern history of prices are accepted, it is necessary to recognize the dichotomy existing between the sixteenth century (*c.* 1540 to 1640) of expansion and the seventeenth century (*c.* 1640 to 1740) of stagnation. But it may well be asked if it is really necessary to accept 'the results of the modern history of prices'. These results are, for the most part, the fruit of arbitrary transformations into precious metals of prices that were originally expressed in local currencies. In fact, they are no longer 'prices' but 'silver prices' and 'gold prices'. And because of this, we are no longer speaking about the history of money, but about the history of 'metals'. Now, so far, all the definitions put forward for the two centuries with which we are concerned are determined by the movement of silver prices. This shows very clearly that there was a price rise in Europe until 1640; this rise was followed not so much by a direct fall but by a stagnation, hence the definitions of the two centuries noted above. But is this movement a good criterion for analysis? I do not believe that it is, and I shall try to show why not in the following pages.

At this point, I should like to remind the reader of a similar misunderstanding which has been established for a different period, that of the fifteenth century. It is significant that, for a long period, historians divided into two distinct groups and argued about the existence, or not, of a crisis (in the sense of a general exhaustion) in the fifteenth century. The reasons for the opposing interpretations centred on the different constructions of the price curves. With silver prices, there is the crisis of the fifteenth century, whereas with prices in local currencies the crisis is dissolved.

Returning to the period that interests us, we see at once that, if the criterion of silver prices is abandoned, the chronological partitions obtained previously are different. For example, in Basse Provence, a long period of price rises ended in 1594; this was followed by a phase of stagnation, which continued until 1689, after which there was another rise, which ended in 1785.[2] How is this to be reconciled with the old chronological definitions of the sixteenth (*c.* 1540 to 1640) and seventeenth (*c.* 1640 to 1740) centuries? Which is right? This is neither the place to solve the debate, nor the time to decide in favour of one or other argument.[3] It merely shows that it is useless to ask the history of prices for what it is unable to give, whether in silver prices or in what we should like to call, to avoid any misunderstanding, 'real prices'. The important point is that it is extremely difficult to try to determine the characteristics of a total economy (that is, in its aspects of production and distribution) on the evidence of prices alone. In addition to this, I should like to deny the truism mentioned above (the discussion of which was postponed). If I deny, at least to a great extent, this function of prices as a 'thermometer' to measure the economy, it is because I

think that one should always argue in terms of a total economy. Prices only act as a 'thermometer' to gauge trends in trade, production and revenue: on consideration, these may have internal discordant elements, and prices could reflect these. I say 'could', for it is by no means certain that they do. The 'thermometer' is, then, subject to change, and cannot measure pure economic reality. It does not pretend to show, with absolute precision, the characteristics, functions and rhythms of the whole. It is because the intervening factors are so complicated that prices cannot accurately reflect the situation in all its intricacy. If we deny the value of silver prices and of their chronology, and accept the 'real prices' and the chronology that comes with them, even then our new periods have no validity in determining the chronological sequence of a whole economic age. Nevertheless, I do not believe that the sixteenth century ends in 1594–5, as the figures for Basse Provence suggest. In this sense, the title of this chapter retains its full significance: researching into the characteristics of a crisis that by its very nature separated two economies, the two 'global' economies of the two centuries.

In order to study the transition from the sixteenth to the seventeenth century, this chapter will concentrate on those elements that are basic to life, curtailing discussion of those that I consider to be only peripheral. The pages that follow can be divided into two distinct sections. The first is characterized by facts and figures that I believe should be put before the reader in order to give weight to my argument. In addition to this, they will clearly show that there is a distinct turning-point between the second and third decades of the seventeenth century. In order to illustrate this turning-point, I have been obliged to take the time-honoured period of 1561–1660 for examination.

As there is only one idea to be demonstrated, and as there is a wealth of evidence, there will be some monotony, which I excuse as inevitable. The second part, which I feel is more animated and 'alive', endeavours to illustrate the internal mechanism of the crisis. It surveys the period 1614–25 and applies to it the observations of the first part. Thus the two sections are linked together rather like documents and commentary. The dynamic nature of the latter will, I hope, compensate the reader for the pedestrian nature of the former.

I
FACTS AND FIGURES

Trade

Turning first to the currents of international trade, there are two major sources of information at our disposal. The first concerns Seville and the Atlantic, and has been imaginatively and diligently researched by Huguette and Pierre Chaunu.[4] The second is that of the Sound, which has been investigated by Nina Ellinger

Bang in a series of volumes[5] which, although they are often criticized,[6] remain of incontestable value, inasmuch as they refer to the (then) contemporary trends.

Let us enlarge our sphere of observation in order to sharpen our judgement. From 1560 to 1650, the total movement of tonnage leaving from, and arriving at, Seville over five-yearly periods, as shown by the Chaunus, is as shown in Table 7.1.[7]

Table 7.1 Tonnage leaving from and arriving in Seville, 1560–1650

Years	*Tons*	*Years*	*Tons*
1561–5	87,048	1606–10	273,560
1566–70	111,039	1611–15	228,705
1571–5	129,480	1616–20	248,214
1576–80	135,926	1621–5	229,828
1581–5	166,057	1626–30	221,522
1586–90	191,589	1631–5	158,414
1591–5	162,323	1636–40	158,475
1596–1600	218,988	1641–5	126,498
1601–5	216,701	1646–50	121,308

Source: Chaunu 1955–7

On the evidence of the tonnage, the turning-point appears clearly in 1610 (exactly in 1610, since in the annual record of tonnage, that is the year that shows the highest level); but up until this point, can tonnage be regarded as a true expression of the vitality of trade? With this documentation, we are in sight, not just of a linear movement in trade which happens to be well defined, but of a system of distribution of American products in Europe, and of the distribution of products from Seville—from Europe, to the new continent. The latter, being in the process of development, needed everything, at least everything that it was able to buy. Because of this, the figures for the value of the merchandise will be more significant than those of tonnage (see Table 7.2)[8]

Now, in the light of the value of trade, which is more significant than its tonnage, a different picture emerges. From 1566 there is a rise continuing until 1596–1600, when it reaches its maximum; then there is a sharp recession during the quinquennium 1601–05, a recovery from 1606–15 and finally a boom in the succeeding five years. But, since Pierre Chaunu has not merely edited a mass of data on the subject, but has also written a shrewd commentary, it would be best to quote him:[9]

> At the heart of the crisis, 1622–23, the Carrera de las Indias was confronted with a thoroughly abnormal situation. The winding-up of an ancient and accustomed prosperity was nearly completed… After 1622, we can say that there are no more turning-points.[10] The break occurred a little after

> 1622, and we could look in vain for something similar at the height of the preceding crisis, that of 1609–13. After the 1609 crisis, the Carrera, although reduced, retained its recuperative abilities, which it lost in the following crisis. Crisis in volume, yes; but also an irreversible change in values. This is the main convincing reason which leads us to place the change in the 1620s, despite the many and excellent reasons for locating it in the structural *pré-crise* of the years 1609–13.[11]

Table 7.2 Value of merchandise from Seville to America, 1566–1635

Years	*Value (maravedis)*	*Years*	*Value (maravedis)*
1566–70	10,828,567,382	1601–5	17,210,271,982
1571–5	10,319,197,700	1606–10	22,945,977,430
1576–80	14,666,341,625	1611–15	20,492,651,323
1581–5	22,059,667,844	1616–20	24,652,885,326
1586–90	20,926,428,610	1621–5	19,744,823,361
1591–5	26,127,615,905	1626–30	—
1596–1600	26,604,787,326	1631–5	13,842,413,722

These passages—and others that I could have cited—expound the trends already indicated by the figures above. They show that the years 1609–13— which we shall consider again and again—saw a real reversal of the general trend, so that we are, as Chaunu affirms, in sight of a structural crisis. I shall have occasion to return to this again.

Let us now observe another dominant sector of international trade in the world of the seventeenth century, that of the Sound. Through the work of Nina Ellinger Bang we can begin to perceive the commercial relationships between two worlds, namely that of the Baltic and western Europe.

We may gain our first impression from the annual average of ships passing through the Sound during the periods shown in Table 7.3.[12] If, in the case of Seville, the figures for tonnage show a turning-point in 1610, here, on the sole evidence of the number of the ships, the turning-point is situated in 1600. This is followed by a recovery, which is sustained (strongly in the second decade) until 1619, after which there is a significant fall.

But what do the above figures indicate? Is the number of ships a good measurement of the flow of trade? Unfortunately, for the Sound, we have neither the total tonnage, nor general data on the value of trade at our disposal. However, through the work of Aksel E.Christensen, we can attempt to make some concrete observations on the commercial relationships between western Europe and Baltic Europe. Christensen has calculated the lastage duty levied by the customs officials at Elsinore. ‘Lastage,’ he rightly notes, ‘was an artificial attempt at joining together the two completely incommensurable and independent

functions: [the] value and volume of commodities.'[13] Now, these figures are expressed as in Table 7.4.[14]

Table 7.3 Number of vessels passing through the Sound, 1497–1657

Period	*Number of vessels (annual average)*	*Period*	*Number of vessels (annual average)*
1497–1547	1,336	1600–9	4,525
1557–8	2,251	1610–19	4,779
1560–9	3,158	1620–9	3,726
1574–9	4,300	1630–9	3,383
1580–9	4,921	1640–9	3,499
1590–9	5,623	1650–7	3,015

Source: Ellinger Bang

Table 7.4 Lastage duty at Elsinore, 1581–1630

Period	*Total*	*Annual average*
1581–90	579.1	57.91
1591–1600	780.7	78.07
1601–10	868.1	86.81
1611–20	1008.1	100.81
1621–30	964.9	96.49

Source: Christensen 1941

Unfortunately, as there are some gaps in the documentation of succeeding years, it is not possible to calculate the total average. Despite this, it is evident that from 1581 to 1620 there is a steady increase, which reaches its maximum in 1619, after which there is a maintenance of a high level until 1623. Then there is a clear fall which continues in succeeding years.

I realize that my evaluation of the figures given by Aksel E.Christensen can easily be attacked. It is significant that after 1618 there was a radical alteration in the customs system of the Sound. This makes any comparison between the figures relating to the years before 1618 and those relating to the years after 1618 impossible. Despite this, each of the two periods (especially that of the years 1618–39) has its own intrinsic value and so we may conclude that after the maximum, reached in 1618, until the end, there was a steady decline.

But other elements make it possible for us to assess the trade of the Sound with more accuracy and more success. First, it is possible to take into consideration, among the total number of vessels passing through the Sound, those that were exclusively Dutch (Table 7.5). They represent almost half the

Table 7.5 Dutch vessels passing through the Sound, 1576–1630

Years	*Total number of Dutch ships*	*Annual average*
1576–80	13,501	2,700
1581–5	11,810	2,362
1586–90	16,072	3,214
1591–5	19,616	3,923
1596–1600	18,009	3,601
1601–5	13,167	2,633
1606–10	16,732	3,346
1611–15	16,877	3,375
1616–20	19,455	3,891
1621–5	13,852	2,770
1626–30	9,110	1,822

total number of voyages, and they express international commercial relationships with the greatest clarity.[15]

It seems to me that the figures show that there are two phases: a first, which spans the maximum of the years 1591–5, and another, which occurs after the depths had been plumbed in 1601–5, in the years 1616–20 when the high levels have been restored. So we have 1616–20, with 19,455 voyages, to set against the old maximum of 1591–5, with 19,616 voyages. These figures become more significant if, as well as the number of ships, their tonnage is considered. It is valued as in Table 7.6 by Christensen.[16] Here, we have a further element

Table 7.6 Tonnage of Dutch vessels in lasts through the Sound, 1591–1640

Years	*Tonnage in lasts*
1591–1600	126,000
1601–10	121,000
1611–20	182,000
1621–30	117,000
1631–40	130,000

confirming my argument. It is possible to cite various others in order to show the clear ‘turning-point’ that occurred in the second decade of the seventeenth century, and which was manifested in the trade between the Baltic and western Europe. The two products that were central to the importance of trade in the Baltic were grain (sent from the Baltic to western Europe) and salt (sent from western Europe to the Baltic). Between 1565 and 1635 grain represented 55 to 65 per cent of the total of commodities bound for the West. Salt represented between 51 and 72 per cent of trade bound for the East during the period 1565–1615, but this fell to between 40 and 48 per cent during the period 1625–35.[17]

Basically, we have two products, which we may legitimately assume are valid expressions of the importance of the Sound, in terms of global trade. If these two leading products are observed, it appears that between 1562 and 1640 the grain trade follows a tendency to increase until 1618 and to decline thereafter. For salt, which was mainly carried in Dutch ships, the trend is the same.[18]

These splendid series of statistics provide clear proof of the turning-point in international trade after 1620. I said 'splendid' series: not in the sense of extensive, or in the sense of being well defined (in the case of Seville especially); but splendid in that they refer to incontrovertible facts. The decline of one port at a certain time may not be very significant. It is always possible, for example, that it will lead to the rise of another. Thus 'malheur des uns, bonheur des autres', or even 'mors tua, vita mea', may apply. This is not the case with our figures. It is possible to argue that the trade of Seville represented the total trade of the vast Spanish Atlantic and that the trade of the Sound represented all commercial communication between the Baltic and Europe. This being so, the decline in the trade of the Sound did not give way to the rise of another trade current. Similarly, the recession of Seville did not precipitate the new life of another port (Cadiz began to become important somewhat later).[19]

The weight of the conclusions produced from the data of the Sound and the Spanish Atlantic is such that it is now possible to take into consideration various specific trade movements in different major centres. These will confirm the conclusions already reached and at the same time will reinforce them.

First we shall enter the Mediterranean world. A preliminary sounding of the rhythms of economic life here can be made by means of a strange document which indicates the number of Dutch ships that were captured and taken to Algiers. During the seven years between 1617 and 1623, 186 ships were captured (to this, 24 ships that were captured and released should be added). We may see the annual distribution of these captures in the following list: 1617:30, 1618:22, 1619:16, 1620:82, 1621:35, 1622:1, 1623:0 1624:4, 1625:1.[20] Is it possible to believe that this is merely a matter of the fluctuating fortunes of war, that it is a reflection of purely military circumstance? Or rather, should it not be taken into consideration that the greater or lesser number of ships captured must have been proportional to the number of Dutch ships present in the Mediterranean? Since it concerns *Dutch* ships, does this not suggest that their presence was a faithful reflection of the favourability, or not, of circumstances? Is it not striking that, in 1620, 82 ships were captured, yet only one was captured in 1622? And this in spite of the fact that in 1622 there was a famine which affected the whole Mediterranean area. It should have constituted a strong attraction for the Dutch ships, which had specialized since 1591 in the transportation of grain from the North to the countries of the South. But a famine is an accidental occurrence. However favourable the economic context may be that it creates, any advantages vanish when a general economic crisis rages.

I believe that it is worth spending some time on the problem of the Dutch presence in the Mediterranean. It is significant that, because of favourable

circumstances initiated by the famine of 1591–3,[21] Dutch ships began to frequent the Mediterranean ports. But they were involved essentially in transportation, since the Italians already had the role of merchants, often supported and protected by the state.[22] It was, however, during these years that the Dutch presence in the Mediterranean became more vigorous and significantly better organized. In 1610 a group of Dutch merchants boasted that they were among the first to organize a commercial enterprise with the countries of the Ottoman Empire; they indicated clearly that before their time there had never been such a commercial relationship with the Mediterranean Levant. They do not give the exact date of their installation in the Levant,[23] and it seems significant that they had always worked under the aegis of the French and the English.[24] It is only in 1611 that the States-General nominated the first consul for Syria, Palestine, Cyprus and Egypt.[25] In this year, a letter to the States-General pointed out that this trade comprised 'A great number of ships whose capital during the previous year amounted to twelve tons of gold'.[26] Meanwhile another report, also of 1611, did not hesitate to define the importance of this trade as superior to that of the East India Company, valuing it at 40 tons of gold[27] per annum.

The turning-point of the Dutch presence in the Mediterranean occurs between 1611 and 1612. In May 1612, the ambassador of the States-General arrived in Constantinople and won the favour of the Sultan by the gifts that he offered him,[28] and in September the first treaty ('capitulation') was signed.[29] At least 12 Dutch ships arrived at Aleppo in 1614, 9 in 1615 and 9 in 1616.[30] In this last year, the tax of 3 per cent on silk was abolished, to the advantage of the Dutch.[31] In 1620 the consul in Aleppo noted that 'trade is increasing'.[32] But, on 23 November 1623, a letter sent to the States-General by a Dutch *orateur* in Constantinople indicated that a combination of factors (rising prices, piracy, higher security costs) had ruined every trade, 'so that for *two years* I have not received any directives from the ports of Smyrna, Cairo, Alexandria and Constantinople'.[33] It could be that this is an example of the usual protests of a diplomatic consular agent, who complained in order to justify a demand for a supplementary subsidy to his government. But this was not how the States-General chose to see the situation in 1625. In order to gild the pill of the 'adverse period of twenty-two months', they declared that 'The previous years have been extraordinarily prosperous', and maintained that, in general, the consul did not have occasion to complain.[34]

But it is not the complaints of consuls or the semi-philosophical consolations of the government that interest us; the important point is that in 1621 the great phase of expansion seems to have suffered a sharp recession, which continued even until 1626, when the consul at Aleppo commented, 'Very few ships are sent now', meaning ships under Dutch jurisdiction. He went on to say, Trade has been declining for the past few years.'[35] This is confirmed by two documents concerning negotiations in the Mediterranean Levant. The first was signed in August 1619 by sixty-three merchants, while a similar document of June 1625 carried only thirty-seven signatures.[36]

But the Levantine situation was not a reflection of general Dutch dealings in the Mediterranean. This is shown by the fact that in the second decade of the seventeenth century Dutch consuls were appointed to the more important places of Italy (Livorno 1612, Venice 1614, Genoa 1615, Sicily 1617).[37] In 1615 twenty-three 'Flemish' merchants were established in Venice[38] and between September 1615 and April 1616, eighty-five Dutch *bertoni* (ships) arrived at that port.[39] In 1619, the consul in Venice wrote to the StatesGeneral, 'It is a long time since I have seen so great a number of ships in the Gulf.'[40]

It seems that this descent of the 'Flemings' on to the Mediterranean began during the last decade of the sixteenth century with transportation; that it was consolidated commercially in the first decade of the seventeenth century; and that it flourished in the next decade. However, by the end of this decade (the 1610s) there was a marked recession. These phases are really only expressions of the Dutch expansion, which in its turn followed the more general phases of Mediterranean trade. If there was a recession in Dutch trade in the Mediterranean, this was not necessarily because Amsterdam itself suffered a setback, but rather because the whole of the Mediterranean found itself in difficulty.[41]

If we now take a cursory glance at Venice, the problems will appear somewhat different, but there should be a recognizable general trend. Let us take the customs duties for a significant period in order to gauge the economic life of this ancient port (see Table 7.7).[42] There are two distinct periods, which I should like to call the preparation, between 1580 and 1595–1600, and the boom, in the following years. It is true that the extreme gap between 1602 and 1621 prevents us drawing many conclusions, but it is still possible to recognize the climax of 1621 and the phase of deflation that followed. But there is another factor that may be particularly significant in the case of Venice. I am referring to the tax on anchorage, as reconstructed by F.C.Lane in Table 7.8.[43]

This series of figures can illustrate the argument that is sustained here in two ways. The first is the clear rise that took place between the last years of the sixteenth century and the year 1609, while the second is the catastrophic fall after 1618 (it will be noted that the great fall of the years 1626–8 was *before* the plague of 1630). What took place between the two phases?

The gap in the excellent table of F.C.Lane is probably destined to stay unfilled, since during these years the state took over 'anchorage' and it was not released until 1618. It may well be asked why the state assumed the management of this tariff in this period. Was it that the 'easy' times were over? Why was the tariff relinquished in 1618? Is it not significant that in Naples as well, in 1619, they ceded the revenue from the customs duties belonging to the king to the state creditors?[44]

Table 7.7 Customs duties in Venice, 1582–1640

		Yield of duties on					
Year	*Wine*	*Exports*	*Imports*	*German traders*	*Maritime customs*	*Olive oil*	*Excise*
1582	168,580	183,000	87,059	4,280	39,500	79,840	9,801
1587	291,157	208,532	81,255	45,998	27,481	61,978	8,615
1594	280,133	227,265	80,235	49,940	91,637	79,500	9,666
1602	293,655	264,724	91,353	63,168	118,658	106,148	11,500
1621	320,624	236,563		42,695	71,000	110,213	10,568
1633	241,191	156,917		26,772	59,112	93,629	8,728
1640	221,673	156,374		24,661	48,705	88,014	6,664

Table 7.8 Tax on anchorage in Venice, 1587–1641

Years	*Ducats*	*Years*	*Ducats*
1587–9	2,006	1620–2	2,701
1589–91	1,760	1622	2,401
1591–3	1,500	1623	1,600
1593–5	1,850	1624–6	1,300
1595–7	2,014	1626–8	750
1597–9	2,513	1628–30	1,000
1599–1601	2,953	1630–2	1,005
1601–3	3,930	1633	1,159
1603–5	6,647	1637	1,160
1605–7	4,760	1638	1,159
1607–9	5,210	1641	1,029
1618–20	1,701	—	—

Source: Lane 1962

After the confident explanation that we have given on the evidence of the figures for the Sound and for Seville, the reader has probably found much of what we have said about the Mediterranean unconvincing. In economic history it is only well-constructed tables of data that are considered valid.

In spite of the paucity of the figures presented for Venice (I could have cited similar ones for the ports of Livorno, Marseilles and Ragusa,[45] but they would have taken up too much space), I should like to look at those elements that affected the Dutch. It is significant that the ‘foreigners’ in the Mediterranean soon reached an enviable prosperity. This was not merely because they were a strong power in a confined sea. It was above all because this was a time of general economic expansion. New centres, notably Livorno and Algiers, established themselves, and not just at the expense of the old centres, which, on the contrary, enjoyed a revival during this period. Similarly, when we see the

Dutch in the throes of a sharp recession the cause is, *as a rule*, to be found in the general difficulties of the Mediterranean. In the Dutch presence, as in their difficulties, nothing depends upon its own contingencies; everything, on the contrary, aligns with the various Mediterranean trends. But let us sail to other seas.

Inside the Baltic, Danzig was the great trading centre, the port on which focuses the hinterland of the Vistula. It is a world that has been thoroughly investigated.[46] S.Hoszowski[47] states, interestingly, that 'between the end of the fifteenth century and the 1620s, grain exports increased twelve-fold (from 10,000 to more than 120,000 lasts), which is eloquent testimony to the dynamic growth of Polish agricultural production and also Danzig trade'.[48] Since we know how important the grain trade was in the general trade of Danzig, the fact that Hoszowski takes the 1620s as the end of his comparison, which spans a long historical period, seems to me significant. This is all the more important when, following an inconsistent historical scheme, he speaks of a 'period of stagnation of Danzig trade, 1650–1772'. What of the period 1620–50? Graph 1 in Hoszowski's article clearly shows that the fall in exports of grain continued throughout the seventeenth century—and beyond.[49] Linked almost directly to this fall in the exports of cereals was the decline of shipping from Danzig passing through the Sound. This is shown in the same graph, and is illustrated by the figures of Table 7.9, which refer not only to Danzig, but also to lesser ports of the region.[50] The fall appears

Table 7.9 Provenance of ships leaving the Sound, 1580–1649

	Provenance of ships leaving Sound				
Period	*Danzig*	*W.Prussia*	*E.Prussia*	*Kurland*	*Riga*
1580–9	12,509	1,152	3,163	366	1,341
1590–9	11,188	1,312	4,566	534	2,183
1600–9	9,589	793	4,270	381	960
1610–19	10,329	748	4,928	322	1,113
1620–9	4,910	410	4,873	280	540
1630–9	5,200	130	2,830	240	2,200
1640–9	6,880	240	2,270	270	2,790

decisively after 1620 for all ports except Riga, whose expansion cannot compensate for the general trend of decline.

In the specific case of Danzig, the fall in the number of ships is accompanied by a sinking in the value of goods traded. This is shown by the imports between 1615 and 1628 (see Table 7.10).[51]

Table 7.10 Value of imports in Danzig, 1615–28

Year	*Value of imports*	*Year*	*Value of imports*
1615	9,300,600	1622	13,239,400
1616	10,164,200	1623	17,455,300
1617	11,614,900	1624	14,486,000
1618	14,411,500	1625	14,894,400
1619	12,904,500	1626	11,073,500
1620	13,860,200	1627	3,665,500
1621	12,672,000	1628	4,386,200

The repetition of this trend is strongly significant, and at the same time it is reminiscent of that already cited for the Sound.[52] This suggests that in this sector of the Baltic there is a clear difference in the periods before and after 1620. There is a turning-point in this year that is so clear as to be indisputable.

The examination of Europe at this time is not complete unless the cases of England and Holland are considered. Unfortunately, the documentation is poor. For England, the only figures at our disposal are those of English ships (along with all other nationalities) sailing from England for the Sound. This, for the most part, reiterates what has been said previously *à propos* of the Sound. In addition to this, we have figures for the export of cloth from London,[53] but these reflect the situation in industrial production rather than that in trade.[54] I shall, however, return to these figures.

The case of Holland is more complicated. The excellent tables of data published by J.C.Westermann[55] concerning Amsterdam begin only in 1624. And, in any case, it would be difficult to approach the 'Metropolitan' commercial life of Holland before this date, as a recent article by I.J.Brugmans makes clear.[56] So, setting aside much of what has already been said about the Dutch presence in the Mediterranean and Dutch shipping in the Sound, I shall consider some other aspects of Dutch economic life later on in this chapter, bringing together those things that will illustrate, albeit indirectly, the movement of trade.[57]

In the world of the seventeenth century, it is diflicult to think of geographical zones closed off one from another. We may presume that geographical distances were very important in the transmission of economic events. If there was a crisis today, for example over shipping in London, this would have immediate repercussions in the ports of America and Asia. We suppose that, in the seventeenth century, this transmission of economic trends was less rapid and less intensive. But in fact we find surprising concordance in places far from Europe with the phases I have so far described. For instance, in Buenos Aires, the imports and exports correspond as shown in Table 7. 11.[58]

Table 7.11 Imports and exports in Buenos Aires, 1586–1655

Period	*Imports*	*Exports*
1586–95	1,810,314	84,758
1595–1605	1,411,282	753,436
1606–15	7,534,123	1,151,678
1616–25	7,957,579	360,904
1626–35	1,792,427	255,974
1636–45	1,708,204	288,196
1646–55	1,875,537	98,500

Source: García 1955

Unfortunately, the figures, published by Juan García, are expressed in decades of years that end in five, thus differing from all the other tables that I have used so far. While I would not dismiss them on these grounds, I cannot really elaborate on them. Is it merely the love of my theory that makes me imagine that the maximum of imports shown in the period 1606–25 would appear even more clearly were it expressed over the two decades 1601–10 and 1611–20? It would give immense weight to my argument if it could be shown by data expressed over decades ending in noughts that the great fall appeared immediately after 1620–1. As it is, the hypothesis can be evaluated by trends in a particular 'product' of Buenos Aires that was essential to trade (and also to general economic development): slavery. The figures in Table 7.12 for the importation of black slaves in Brazil (they were brought from the coast of Africa to Brazil by Portuguese merchants)[59] are highly significant and are presented here not merely for decades, but for each year. (This is necessary to avoid doubtful interpretations.)

It seems clear to me that the boom, after 1606, reaches its maximum in 1621. I stress that an enterprise such as the slave trade was, during the early years of the seventeenth century, of crucial importance to economic development. The great demand for slaves shows that the city was, until 1621–3, experiencing an extraordinary phase of expansion in all aspects of its economic life. Now, is it merely accidental that the highest number of slaves imported appears in 1621, while a little after this date the decline continues inexorably, despite such a dynamic start?

Is this an exception? I do not believe that it is. The documents for the study of Spanish America are many. There are *almojarifazgos*, *alcabalas* and other customs regulations, which exist in great quantity and with which Pierre Chaunu has dealt very successfully.[60] But with the aims of this chapter in mind, I do not know how far this type of documentation truly reflects the reality of the economic trend *ad annum*. It is a good guide for the long term, but I very much doubt whether it can be used to study short-term trends, or, more simply, to observe the profound change in commercial activity.

Basically, the figures for the money exacted for payments of *almojarifazgos*, *alcabalas*, etc., are valid only in so far as they show general rather than commercial aspects of the fiscal finances. It would be necessary to examine the duties themselves in order to know which precisely were taxes of imposition. It is possible to affirm that in the long term the figures are strongly representative of the general tendency, but they may not deceive us into finding in them the statistical documentation of a 'turning-point'. However, Pierre Chaunu, with his knowledge of this type of source, maintains with regard to the 'Pacifique des Ibériques' that there is:

> nothing, absolutely nothing, that would not fit into the most classic models of the European conjuncture and, *a fortiori*, Seville and her Atlantic trade. Pushing the analogy beyond what is undoubtedley reasonable, we observe that the most important date here is at the end of the 1610s—broadly speaking, 1618–20; more specifically, 1619. That is to say, the date at which the volume of Seville's Atlantic trade began to fail.[61]

Since we have been wending our way in a kind of round-the-world tour, let us continue the journey. Excellent guides for us will be C.R.Boxer,[62] and, once more, Pierre Chaunu.[63] They will take us to Macao, the port that opened up the world of continental Asia. Macao, being Portuguese, was in opposition to the Spanish Philippines. They were two 'worlds', which, though they detested each other, were often obliged to sink their differences. Their two roles became defined through this *concordia discors*. Now if, as we have already seen for Manila, 'the downturn in the secular trend occurred in the second decade of the seventeenth century'[64] (that is for commercial activity, excluding the Japanese), this suggests that this 'either would remain at a high level, or even continue to increase, albeit slowly, between 1612 and 1619'.[65] It is possible to cite the same cause for Macao's difficulties, which began during the period 1615–20 and were accentuated after 1621 with 'official renewal of the war with the Dutch'.[66]

Frankly, I do not think that the influence of the war was very great. In the case of Macao, Pierre Chaunu considers the revival of hostilities as an element bringing about the end of the boom. At the same time, the Dutch attributed much of their misfortune to the same war.[67] They referred, above all, to the rivalry of the English, and it seems certain that the English also found themselves in difficulty, because of the Dutch or for some other reason. It should be noted that, because of all the complications, the inconveniences and the recessions that occurred during the 1620s, merchants, politicians and bankers had a real need to rationalize the causes of the difficulties encountered. We ought not to pay too much mention to their complaints. It seems to me that we should look not just at those economic aspects that brought about the structural change, but at the collective mentality as well. The men of the time did not face up to the real situation in which they found themselves, and they believed the crisis to be of the kind that often appeared.[68] They did not expect it to last long. In fact, they were faced at this time by a genuine, structural crisis of the sort that changes the character of an epoch.

Table 7.12 Importation of black slaves in Brazil, 1606–55

Year	*Number of slaves*	*Year*	*Number of slaves*
1606	517	1631	339
1607	226	1632	470
1608	39	1633	297
1609	60	1634	78
1610	37	1635	187
1611	348	1636	175
1612	666	1637	225
1613	780	1638	145
1614	831	1639	211
1615	1,089	1640	295
1616	14	1641	175
1617	9	1642	0
1618	8	1643	0
1619	218	1644	0
1620	760	1645	0
1621	1,834	1646	0
1622	630	1647	0
1623	718	1648	0
1624	74	1649	303
1625	106	1650	0
1626	73	1651	10
1627	20	1652	0
1628	189	1653	0
1629	186	1654	2
1630	334	1655	0

Industry

Turning to industrial production, it is difficult to undertake a cursory observation of industry during the transitional period of the sixteenth and seventeenth centuries. There are a number of reasons for this, but I shall cite just some of them. In the first place, there are not usually many extensive tables of data on industrial production. But even when we have long columns of figures at our

disposal, it is easy to raise objections to them. In few sectors other than the industrial can the success or failure of a city's economic activity be interpreted as the success or failure of that city alone. To give a simple example, if the cloth industry declines in Florence, it is always possible to argue that Prato, Siena and the other minor centres will increase their cloth industries so that the 'Tuscan' balance sheet ought to stay the same. The same argument can be applied for various sectors of production. If the production of woollen cloth declines in a certain locality, that of silk may undergo considerable development in compensation. In addition to this, it is almost impossible to find tables of figures (production keys) for industry, of the sort used for Seville and the Sound in the sphere of trade, that may be a measurement of so wide a sector.[69]

We should mention the intrinsic limits of industry during these centuries. What in fact is 'industry'? It is essentially textile productions, mineral extractions and shipbuilding. The rest of industrial activity is of the individual, artisan variety. I stress that, however important all these forms of activity may be, they do not represent more than a tiny percentage of world production at this time. The considerations that follow must be accepted (if, indeed, they are accepted) with one important reservation in mind: the role of industry in the development of economic trends was always one of acceleration, not of traction. In early modern Europe, industry could speed up growth but it could not start it.

First, let us see what materials we have at our disposal. The first source is that of textile production at Leiden as shown by N.W.Posthumus in his work[70] which is one of the greatest pillars of economic history, and one that should be a model. Then there is the production of cloth in Venice,[71] the textile industry of Florence,[72] the cloth exports from London[73] (the last figures are more an expression of production than of distribution, so we are not simply studying commercial movements); the total consumption of mercury in Spanish America and the production of this metal in the mines of Huancavelica[74] (expressions of the mining industry of that continent); the production of silver from the mines of Potosí;[75] and finally other figures, more or less complete and representative of other trends in assorted sectors.

We shall begin with the case of Venice and the production of woollen cloth. This is a particularly significant case because the output increased from 1,310 pieces of cloth in 1516 to 28,729 pieces in 1602, following a steady rhythm of increase. After that, it fell to 1,721 pieces in 1712 with an equally steady rhythm of decrease. For the first time in this chapter, we have a series of figures that does not show a clear boom in the first twenty years of the seventeenth century. Neither do they indicate a clear progression or even a sustained movement. But I hope that my argument will not lose force because of this.

The important point (I mention this for the first time here, but shall return to it) is that we are not seeking to find in these figures curves rising to dizzy heights, thereafter to fall. Our aim is to observe the differences in the speed of growth at different times. The production of woollen cloth is shown (in decades) in Table 7.13.[76] A good way of penetrating beyond the superficial would be to

calculate the average percentage increase or decrease over the decades during the period 1561–1660 (Table 7.14).

Table 7.13 Production of woollen cloth in Venice (in decades), 1561–1660

Years	*Cloths*	*Years*	*Cloths*
1561–70	171,946	1611–20	192,833
1571–80	178,403	1621–30	162,999
1581–90	194,459	1631–40	123,677
1591–1600	223,804	1641–50	112,902
1601–10	216,701	1651–60	97,064

Table 7.14 Average percentage increase or decrease in production of woollen cloth in Venice

Years	*Increase or decrease over preceding decade %*	*Years*	*Increase or decrease over preceding decade %*
1561–70	+2.66	1611–20	+15.13
1571–80	+6.52	1621–30	−7.61
1581–90	+16.11	1631–40	−26.16
1591–1600	+33.63	1641–50	−32.59
1601–10	+20.39	1651–60	−42.05

These figures show clearly the profound trends over the two centuries. It is evident that there were positive phases until 1620, even though the maximum increase came during the period 1591–1600. After this date, the potential capacity of the industry remained the same until 1620.

But I stress that Venice is a special, limited case. We are, in fact, in front of a descending curve;[77] this is shown clearly after 1590. However, after that there was no real structural fall. This did not occur until after the decade 1611–20.

Confirmation of much of what has been said may be found in the number of silk looms in operation in Venice between 1592 and 1624.[78] If the totals are compared, it is impossible to discern anything other than a tendency to increase between 1592 and 1602 and a period of stability in the years following. But this total is a false indication, in that it includes a number of looms associated with the production of *ormesini*. Now there is no doubt that Venice, during the sixteenth century, had tended to abandon this sort of cloth (mainly to concentrate on the production of brocade, which was more profitable). If we look at the looms engaged in the production of brocades and silks, we have the figures shown in Table 7.15.[79] There is a clear increase, which finds its maximum in 1619, to be followed by a significant drop. Again, the figures correlate with a chronology that is indisputable.[80]

The case of England is of particular interest. We have not, I repeat, the figures for the textile production of this country at our disposal, but we may take a first

measurement of this production through the commercial trends expressed by wool exports from London (Table 7.16).[81]

Table 7.15 Looms engaged in production of brocades and silks in Venice, 1602–24

Year	*Looms*	*Year*	*Looms*
1602	1,722	1614	1,522
1603	—	1615	1,463
1604	1,342	1616	1,334
1605	1,179	1617	1,464
1606	1,174	1618	1,505
1607	1,255	1619	1,910
1608	1,388	1620	1,591
1609	1,355	1621	1,266
1610	1,400	1622	904
1611	1,335	1623	—
1612	1,498	1624	1,563
1613	1,662	—	—

Table 7.16 Wool exports from London, 1571–1640

Years	*Woolsacks exported*	*Year*	*Woolsacks exported*
1571–3	73,204 (ave.)	1606	126,022
1574–6	100,024 (ave.)	1614	127,215
1577–9	97,728 (ave.)	1616	88,172
1580–2	98,002 (ave.)	1618	102,332
1583–5	101,214 (ave.)	1620	85,741
1586–8	95,087 (ave.)	1622	76,624
1589–91	98,806 (ave.)	1626	*c.* 91,000
1592–4	101,678 (ave.)	1627	*c.* 88,000
1598	100,551	1628	108,021
1598–1600	97,737 (ave.)	1631	84,334
1601	100,380	1632	99,020
1602	113,512	1633	80,244
1603	89,619	1640	86,924
1604	112,785	—	—

The frequent gaps prevent us from reaching precise conclusions; neither can we make an exact distinction between the ‘commercial’ and ‘industrial’ aspects of the trend.[82] Besides this, we lack the data for some years (1619 and 1621 in particular), which are very important in the specific case of England.

However, there are other means available to us. We may look at English cloth carried by ships of all nationalities through the Sound (Table 7.17).[83]

Table 7.17 English cloths carried through the Sound, 1591–1690

	Cloths	
Years	*Total*	*Annual average*
1591–1600	287,000	(28,700)
1601–10	320,000	(32,000)
1611–20	312,000	(31,200)
1621–30	281,000	(28,100)
1631–40	271,000	(34,000) (only 8 years)
1641–50	207,000	(20,700)
1651–60	87,000	(12,420) (only 7 years)
1661–70	150,000	(15,000)
1671–80	163,000	(16,300)
1681–90	209,000	(20,900)

What stands out in this group of figures is that from 1590 to 1610 there is the clear, characteristic increase, followed by a period of stability during the second decade of the seventeenth century. Then there is a fall, followed by a revival during the 1630s and after that a very significant fall. But it will be necessary to look at other figures, which will show that, in all the tables compiled by Supple over the period 1591–1693, the absolute maximum was reached in 1623 with 60,000 pieces, and the minimum (9,000 pieces) was recorded in 1629.

Another argument of fundamental importance should be taken into consideration. I am referring to the strengthening of the customs system of the Sound, which occurred in 1618. Because of this, Supple's table of figures should be divided into two sections, on either side of this date. The figures of the second section reflect quite accurately the number of pieces of cloth carried. In the first section, however, the influence of smuggling was so great that the figures given are almost certainly inaccurate. However, if a (hypothetical) increase of 30 per cent smuggling is allowed, the correct totals appear as shown, in Table 7.18.

In the periods that follow, the original figures are correct. At this point, and only at this point, the 60,000 pieces of cloth of 1623, indicated by the annual trend,

Table 7.18 English cloths carried through the Sound, 1591–1620 (including smuggling)

Years	*English cloths passing Sound*
1591–1600	37,310
1601–10	41,600 annual averages
1611–20	40,560

Table 7.19 Apprentices and freemen in the Shrewsbury Drapers' Company, 1572–1660

Years	*Apprentices*	*Members*
1572–6	20	24
1577–81	7	14
1582–6	7	11
1587–91	1	12
1592–6	4	5
1597–1601	19	5
1602–6	14	—
1602–7	—	6 (6 years)
1607–9	6 (3 years)	—
1609–10	8 (2 years)	—
1608–10	—	24 (3 years)
1611–15	29	11
1616–20	43	22
1621–5	16	21
1626–30	27	17
1631–5	34	21
1636–40	25	14
1641–5	16	14
1645–50	38	15
1651–5	46	32
1656–60	36 (4 years)	12 (4 years)

Source: Mendenhall 1953

acquire their full significance. Similarly, what had seemed to be a maxi mum during the decade 1631–40 appears instead as a steady period of stagnation.

I am quite aware that the authority of these figures can be challenged, especially if my corrections are not accepted. It would, however, be possible to question the smuggling figure that I propose without rejecting the conclusions established in many critical works of the last few years that are concerned with the problems of the Baltic. However, other indicators show that the rhythm of productivity in the English textile industry underwent a change between the second and third decades of the seventeenth century. The number of apprentices and freemen in the Shrewsbury Drapers' Company developed as shown in Table 7.19.[84]

Again, I insist that the real problem is not that of finding the high points and low points. The idea is not to lead these figures into some pre-ordained pattern. What it is important to realize is that the maximum number of matriculated apprentices is reached during the years 1616–20 and that the maximum will not be reached again until the years 1651–5. The same trend is found in the matriculation of members.

It seems to me that the clearest confirmation of the 'global' development of European textile production is to be found in the case of Holland, and particularly in the magnificent study of the Leiden cloth industry by N.W. Posthumus. Rich in data and full of intelligent commentary and explanations, it makes clear the development of production in this key textile centre. The total production of wool, which it is possible to estimate only after 1591, shows a clear upward movement, which reaches its first maximum in the decade 1621–30. There is a marginal decline in the following twenty years and a recovery (which moves back towards the old maximum) in the last decade studied.[85] But perhaps it is possible to study this trend in a different way, following, instead of doubtful figures, the percentage increase or decrease (Table 7.20).

Table 7.20 Percentage increase or decrease in production of wool in Leiden, 1591–1660

Years	*Increase or decrease %*	*Years*	*Increase or decrease %*
1591–1600	—	1631–40	−1.14
1601–10	+33.66	1641–50	−9.81
1611–20	+30.54	1651–60	+18.83
1621–30	+14.34	—	—

Source: Posthumus 1908–39

Here we see that the two highest rates of development occur during the first twenty years of the century, to be followed by a notable decline. This, in turn, is followed by a recovery, lacking, however, the force of a 'boom'. There is evidence of a significant recovery in an excellent graph published by Posthumus.[86] This shows clearly that the maximum of 1619 is followed by years of fluctuation until 1653, when the final increase occurs, which continues until 1664.

It is possible to maintain that the number of pieces of cloth produced is not a very significant guide; that what really matters is the total value, and the quality (poor or luxury) of the wool produced. It seems clear, following the qualitative details of the figures for total production,[87] that the changes that are shown are all to the advantage of the poorer goods.[88] This element reinforces the impressions that we have already gained from the percentages of increase or decrease shown by Table 7.20.[89]

Much of what has been said *à propos* the textile industry is confirmed in other sectors of production in Holland. The production of soap in Amsterdam is shown in Table 7.21.[90] Thus, it may be said that production reached its height between

1617 and 1620. This maximum was attained through a steady growth, which began in 1595 and was over by 1620.

Table 7.21 Production of soap in Amsterdam, 1595–1630

*Years**	*Tons of soap*	*Years**	*Tons of soap*
1595–1600	140,787	1615–20	200,746
1600–5	139,900	1620–5	177,469
1605–10	157,639	1625–30	159,798
1610–15	180,030	—	—

Note: * The years run from 1 August to 31 July

The other sector that is outstandingly important in the case of Holland is that of shipbuilding. In Rotterdam, the number of ships built rose from 20 in 1613 to 30 in 1620, 23 in 1630, 23 again in 1650, falling to 11 in 1673 and 5 at the beginning of the eighteenth century.[91] It is true that the long series of figures over the period 1613–1700 is marginally less significant because of the concentration of this activity in Rotterdam itself on behalf of other ports, but in spite of this, the increase in the number of ships built between 1613 and 1620 and the decrease in those built between 1621 and 1630 cannot be ignored. It was not only the dockyards that increased their productivity: the industries that were associated with shipbuilding, such as rope-making and saw-milling, developed in the same way.[92]

It was during the first twenty years of the seventeenth century that conditions were ripe for the extraordinary industrial development of Amsterdam. It was then that the premises were created (which soon became more than premises) for the sugar-refining industry,[93] for the glass industry[94] and for the distillation of alcohol.[95] There was immense enthusiasm and animation, which was reflected in the influx of new citizens to Leiden, Amsterdam, Middelburg, Delft and Gouda (Table 7.22).[96]

As in the case of industrial production, we find an increase in population that continued steadily until 1620. After this, the level fluctuated somewhat and in some instances rose further than before. But despite this, the driving force was gone, especially in Amsterdam. These figures should be seen as expressions of urbanization,[97] which means that they are more important than any of the other tables concerned with industrial production. Basically, if the migration rate to the towns is reduced, or at least stabilizes, this means that the town is offering fewer prospects of work. It is surely significant that in a centre such as Amsterdam, where there had been no real textile industry (apart from the preparation of imported unbleached cloth) since the middle

Table 7.22 New citizens admitted to Leiden, Amsterdam, Middelburg, Delft and Gouda, 1575–1659

	Number of new citizens admitted				
Dates	*Leiden*	*Amsterdam*	*Middelburg*	*Delft*	*Gouda*
1575–9	193	334	437	146	69
1580–4	328	440	426	165	108
1585–9	400	724	939	191	126
1590–4	580	853	1,064	205	72
1595–9	353	973	676	158	51
1600–4	314	1,481	382	138	95
1605–9	424	1,822	396	142	110
1610–14	485	2,605	340	176	115
1615–19	577	2,768	334	124	173
1620–4	488	2,703	354		
1625–9	414	1,986	290		
1630–4	337	1,987	317		
1635–9	579	—	348		
1640–4	609	—	391		
1645–9	596	—	386		
1650–4	605	—	390		
1655–9	799	*c.* 1,900	599		

of the sixteenth century, there was a revival of woollen cloth production between 1617 and 1619. The municipality even offered assistance to the sergeproducers at the end of 1619. Can this be seen as a mere coincidence, or do these dates of recovery coincide with the others, so linking them to the wider economic trend?[98]

Finally, before proceeding to another argument, I should like to point out that a certain similarity exists between the figures shown above and those for the members and apprentices of the Shrewsbury Drapers' Company.

Are these trends peculiar to Holland, or can they be found throughout the wider economic spectrum? There are many factors that suggest the latter. There were sugar refineries flourishing in other centres at this time, for example in Copenhagen.[99] And once again, we have what is probably the best testimony in the well-documented textile industry. In Hondschoote there is a sustained rhythm of exportation (of almost all production) during the period 1590–1630. As in the case of Leiden, it is important to realize that the first maximum (57,310 pieces) was reached in 1618, as a result of the boom, whereas the second maximum (60, 730 pieces) was reached during a decade when the production level was already high. Thus the percentage increase of the second maximum is less than that of the first.[100]

The various minor centres of Flanders and Germany,[101] though not statistically documented, seem to follow a parallel movement of a boom during the last few years of the sixteenth century and the first twenty years of the seventeenth. Any increase thereafter was not due to this early trend. In addition to this, we may assume that the constriction of the economy during 1619–22 was clearly manifest in this region, which in time was to become one of the greatest industrial centres of Europe.

Once again, we shall sail the seas. The continent of America should prove an excellent target for our observations. Let us study the total consumption of mercury; this will give us a most accurate insight into the mining industry of Spanish America, since from around 1570 all silver extraction was done by the process of amalgamating the mineral silver with mercury. This last was brought from Europe in large quantities, but was also produced locally, in the mines of Huancavelica in Peru. The figures for mercury mining have been tabulated by Pierre Chaunu with his customary accuracy. Here, I am limited to producing those shown in Table 7.23.[102]

Table 7.23 Mercury mining in Peru, 1561–1660

Years	*Production of Huancavelica (quintals)*	*Total consumption production plus imports (quintals)*
1561–70	—	8,750
1571–80	33,974	56,462
1581–90	67,786	94,470
1591–1600	65,755	94,886
1601–10	35,355	70,709
1611–20	61,896	106,675
1621–30	39,857	108,388
1631–40	48,717	95,818
1641–50	53,350	68,174
1651–60	67,260	93,280

What may be deduced from these figures? If the column showing the total consumption is studied, this gives the impression that the maximum is reached during the decade 1620–30. In fact, this is false, since it must not be forgotten that these figures include the quantities sent from Europe. Now, as ‘the mercury exported from Seville could only be used two or three years later’,[103] this creates chronological alterations in our quinquennial averages. If, on the other hand, we look at the production of mercury in Huancavelica alone, it is evident that the maximum is established in 1611–20, with 61,896 quintals (there is also an unexpected recovery from 1651–5).

We may now consider the production of silver in the mines of Potosí. The reader may think it odd to mention Potosí in this context of industrial production,

since if Potosí is discussed it is always *à propos* of Europe's money supply. But in the former sector, as in the latter, there are the customary concordances and discordances. As far as production was concerned, it eventually became difficult to exploit the metal and it involved high costs. Hence the decline of the argentiferous possibilities of America, which goes far towards explaining why the seventeenth century was a 'century of economic stagnation'. In response to such arguments, it is sufficient to remember that the production of the American silver continued, though on a reduced scale, during the stagnation in question. In addition to this, the reduced scale of silver production was not the cause of stagnation, but only a reflection of it. Economic historians often seem to forget that the production of precious metals is not so much proportional to the wealth of the deposits as to the needs, in terms of precious metals, of the world economy at different times. Gold and silver are extracted because they are necessary, not because mines happen to be discovered at a certain time. Mines are 'found' when they are needed; there is not some casual discovery of them (the same applies to geographical and technical 'discoveries').

It is in the light of these considerations that we should study the figures in Table 7.24, indicative of the 'reales quintos y 1½ per cent de cobos' (levies on

Table 7.24 Levies on the production of the mines of Potosí, 1561–1660

Years	*Tax revenue*	*Increase or decrease %*
1561–70	4,205,379	−58.06
1571–80	5,811,456	−43.47
1581–90	13,729,510	+33.66
1591–1600	14,458,165	+40.75
1601–10	13,656,749	+32.05
1611–20	12,022,943	+17.05
1621–30	10,597,768	+3.17
1631–40	10,870,266	+5.82
1641–50	9,391,833	−8.57
1651–60	7,971,170	−22.40

the production of the mines) of Potosí between 1561 and 1660.[104] Once more, the turning-point seems to occur during the period 1591–1600, but again, we should remember that we do not seek mere records of figures but real, structural changes. If the figures of wastage, averaged throughout the century are studied, it is evident that the decline sets in only after 1620.

Agriculture

We shall now turn to agricultural production. There is no doubt that, in the present state of historical knowledge, a discussion of agriculture, agricultural

production and the peasants' life is a difficult, one may even say risky, under taking. It will be necessary to circumvent this difficulty in order to show the panorama of economic events between the sixteenth and seventeenth centuries, as they concerned the majority of the people, and not just the small minority of merchants, bankers, industrialists and the few who moved in their circle.

I should stress (even if the reader is aware of the relevant bibliography) that I am not able to produce very much statistical documentation in support of my argument. Unfortunately, there are few tables of data showing increases or decreases in production, movements of land reclamation (or flooding), or the contrasting trends between cereal cultivation and cattle-rearing. It will be necessary to expound theories in a general way, to make general references, which may be supported by some not altogether reliable figures. We are studying a sector where progression is slow, less slow than is commonly believed, but still not rapid. Hence, we shall not expect to find a 'turning-point' in the span of so few years. It will be a matter, rather, of showing the chronological relationship between the weakening of the agricultural structure and the effect of this upon the structure of industry and trade.

There is no doubt that, throughout the sixteenth century, agriculture was sensitive to price rises and to demographic expansion. Thus, we have two good reasons for the boost in the cultivation of agricultural produce, especially grain. The price rise secured the increased value of crops and land, while the population growth guaranteed a good market and provided the labour necessary for this increase in production. At the same time, the production increase accentuated the price rise. In the absence of technical innovations,[105] this increase in production was obtained by placing more land under cultivation. Assarts, land reclamations, and deforestations were all factors in European agrarian life during the sixteenth century in Venetia, Tuscany, Holland, France and England. In fact, there was scarcely a part of Europe that was not engaged, at this time, in placing new land under cultivation.[106]

With the exception of southern Italy, Europe shows a marked tendency to limit the space set apart for the rearing of animals. In this way, cereal cultivation was increased. The innovation was widespread in Europe and was most commonly established in the flatter parts of France, Holland, Spain and Germany. Europe had thus been able to cope with the exigencies of producing food over the years, and at the same time agricultural production had improved. Not only was cereal cultivation increased, but new varieties of cereal were introduced (such as rice and maize) and (in the case of rice) were produced on a larger scale. However, if production had increased, there seems little doubt that this increase was accompanied by a clear deterioration in the peasant class, which lost privileges and ancient constitutions. But, looking at the balance sheets at this time, it would be quite in order to speak of an agricultural expansion in the sixteenth century. It was at the end of this century and at the beginning of the next that the situation deteriorated. I believe that throughout the sixteenth century

(though the phenomenon had already begun towards the end of the fifteenth century) many engaged in agriculture were drawn to speculative enterprises. The reclamation of land entailed the investment of money in precarious operations, which in spite of all precautions (political, technical and economic) might come to grief. In comparison with secularized church lands, bought by similar means, land reclamation was an insecure investment. It required much work and money to make land that had been badly neglected productive once more. This phenomenon of 'returning to the land' (much cited in the case of Italy) is not regarded by some historians as a sign of apathy in a spirit once adventurous, but simply as a different manifestation of the same spirit in the practice of speculation.[107] We are discussing an agricultural economy of the sixteenth century, which is 'capitalist' and 'bourgeois' (though we should not forget to put these words in inverted commas). During the course of the sixteenth century, what was 'bourgeois' at the beginning of the century was transformed into 'noble' at the end of it (this was valid only for proprietors of sufficiently vast lands); each great 'bourgeoisie' was destined to be turned into 'nobility'. Whereas an ancient nobility could be adventurous and sensitive to the problems of their time, the new nobility, with the conceit born of their ersatz title, could not be other than reactionary, or at best conservative.

These great phenomena, one of which Fernand Braudel has called the *'trahison de la bourgeoisie'*, and the other *'réaction seigneuriale'*,[108] did not comprise two distinct classes for very long. There was basically one class, comprising more or less the same people, who were at least the descendants of the same families. Thus, they brought their bourgeois origins and, above all, their bourgeois functions and offices into the *réaction seigneuriale*, a reaction that, in the case of Italy, I have termed 'refeudalization'.[109]

I believe that this word may be applied not only to Italy and the Mediterranean, but also to a much wider area of Europe, with the exception of Holland. I believe that the new nobility, together with the descendants of the old, considered it possible that the profits made by their ancestors in commercial operations could be equalled by their own diligence and wise investment in the 'refeudalization' of landed property

But what is meant, exactly, by this word? It does not refer merely to the restoration of a juridicial or a political system but primarily to an economic system. If the old privileges were not revived *de jure*, they came to be reestablished *de facto*. Since the privileges are liable to abuse, coercion became the inevitable 'currency', which replaced the old, 'bourgeois' metal coinage. Land reclamations made in the seventeenth century were paid for not with money, but with corvées, feudal services and seigneurial demands.

It is possible to conclude that 'agricultural production, in contrast with all other production, decreased very little in the seventeenth century'.[110] It has been stated in relation to northern Italy, but I believe that it may be applied to almost all of Europe, that it was not so much agricultural production that remained constant, as the total return of the land (that is, the purely economic profits, and the value—

difficult to measure in figures—of services due to the lord) that remained constant.

For the lord in the seventeenth century, economic affairs favoured him in that many of the services owed to him were free. If the availability of manual labour had increased (reflecting a general demographic contraction), it was possible to reduce crop cultivation and increase animal-rearing. And if new land was required, there was always the common land. These alternating movements in crop cultivation and animal-rearing are a means of gauging the rhythms of agrarian life. There seems little doubt that the increase in animalrearing was to the great detriment of agriculture. It should not be forgotten that animal-rearing requires fewer people; the tilling of fields requires many more. It is enough to consider the alterations in cereal cultivation and animalrearing (and the social consequences that these entailed) in the Argentine Pampas during the nineteenth and twentieth centuries, to gain some insight into this situation. In the Europe of the sixteenth and seventeenth centuries, it seems clear that the sixteenth century saw the development of cereal production, and the subsequent decline in animal-rearing, while the reverse is true for the seventeenth.

Animal-rearing is a sign of widespread poverty. It signifies that there are problems in cultivation, and it necessarily subjects the people to a hazardous life-style. The lord may have been able to derive great benefits; the peasant masses could not. For the purposes of the total economy, it is not the level of the lord's profit that determines the well-being of an economic organization. It is rather the average standard of the peasant's life, and his monetary situation, that serves as an indication.

Let us leave these generalizations and look instead at three detailed cases that seem to me to be very important. The first is that of the Company of the Millers of Cesena, which has been illustrated in an excellent book by Antonio Domeniconi.[111] Their case seems particularly significant, in so far as milling is an activity that is central to three economic sectors: agriculture, trade and industry. We find that the number of 'shares' in the company increased from 28 in 1418 to 120 in 1591 and to 149 in 1640. This level was maintained until the end of the century. But what seems to me to be more significant is that, while the number of 'shares' in the period under consideration increased regularly at least until 1640 and stagnated after this date, the number of holders showed a tendency to increase (indicating an expression of the dispersion of capital) until 1600, after which there is a contraction (signifying the concentration of capital) which continues to the end of the century, despite the brief recovery of 1650–60. This gives the impression that, in the first decade taken into consideration, more people (nobles and artisans) showed an interest in this kind of investment, an interest that later diminished. The figures show that:

1 Until 1600 there was widespread interest in investing money in this enterprise.
2 Between 1600 and 1620, this interest was somewhat uncertain.

3 After 1620, there was general determination not to risk anything more.

We may find another example in the rural benefices of the five bishoprics of Cracow, Plock, Poznan, Wroclaw and Gniezno, in Poland. Between them, they included 1,400 villages, 46 cities and about 300 ‘seigneurial domains’, which have been studied recently in the enthusiastic and intelligent work of Leonid Zytkowicz.[112] It is possible to discern in the contradictory texts certain basic characteristics. During the course of the sixteenth century, the peasants were able to save money, but after the end of the century there was a progressive and widespread pauperization. This is shown by the fact that in the sixteenth century, the peasants were able to redeem a good part of the duties owed to their lord by making payments in money. In the period after this, they were unable to do so. In the case of Wolborz, in 1604, more than 50 per cent of the peasant landholders were free, while the inventory of 1623 showed a clear diminution in their numbers. This may have been because the lord renovated (in order to make more efficient) the system of corvées: or it may have been that he conceded the tax at a particular time. The old types of corvées were very rigid (for example, the corvées of transport) and were a great burden on new developments at the time (the increasing trade with foreign ports and the number of workshops created for the production of beer).

These interchanges between landlord and peasant could not but have had their effect on the harvests, so that between 1532 and 1583 we find an increase of about 50 per cent on seeds planted in the domains of Kujawy, in the bishopric of Wrocław.[113] We do not know what followed in this area, although all the factors point to a contraction in production. But, we may be enlightened by the excellent work of Jerzy Topolski, which considers the rural economy of the domains of the bishop of Gniezno, showing that in the first half of the sixteenth century the increase in crops was considerable, was less so in the second half, while the first half of the seventeenth century saw the beginning of a decline that, by the second half of the century, had turned into a drastic regression.[114]

It is possible to object that much of what has been said so far refers to ecclesiastical property. Leaving aside the fact that this last is a good representation of the general conditions of agricultural property in Poland, it remains true that if the agrarian economy of the rural domain of the city of Poznan is studied, we find the same trends, during the period 1582–1644, as elsewhere.[115] The most striking confirmation comes, it seems to me, from the work of Andrzej Wyczański, somewhat dubious regarding method, but full of vitality, intelligence and considered conclusions.[116] This indicates that between 1564 and 1660, production of four cereal crops (wheat, oats, barley and rye) in 160 agricultural enterprises, throughout the six regions of Poland, show almost complete stagnation, which is followed, after 1660, by a drastic fall, attributed mainly to the war.

So, the case of Poland shows that there was:

1 An expansion of production until the end of the sixteenth century, which was followed by a stagnation during the next few decades, which in turn was followed by a sharp recession.
2 A certain well-being (obviously relative) in the peasant class throughout the sixteenth century. The lord in this century was not a philanthropist, but there were possibilities for the majority of peasants to improve their lot, for example, in the commutation of their duties.
3 A reaction that, I stress, did not follow a halcyon period, but that was so oppressive and indiscriminate that it was, in effect, a regression to serfdom. It appeared to run parallel in the seventeenth century, with the contracting of cereal cultivation.[117]

Finally, we have the third example, that of land reclamation in Holland. The struggle against the sea is crucial in the economic history of Holland— and in the history of the whole Netherlands. The intensity of the war waged by the Dutch to keep the land out of the sea, or the marshes, reflects the vitality of agricultural life in their country. Between 1540 and 1864, the figures for the reclamation of the land appear as shown in Table 7.25.[118] A similar

Table 7.25 Land reclamation in Holland, 1540–1864

Years	*Index**	*Years*	*Index**
1540–64	346.0	1715–39	100.0
1565–89	75.3	1740–64	94.8
1590–1614	339.9	1765–89	168.3
1615–39	418.5	1790–1814	148.8
1640–64	273.0	1815–39	160.6
1665–89	115.7	1840–64	368.0
1690–1714	117.6	—	—

Source: Slicher van Bath 1960 and 1955
Note: *1715–39= 100

trend is shown in the cultivations of East Frisia between 1633 and 1794 as shown in Table 7.26.[119] These two sets of figures show that the recession comes only after the period 1660–70, in contrast to the previous cases, where the signs of recession are already present at the end of the sixteenth century. In addition to this, looking at the figures for the Polders, we see that this reclamation began on a large scale only at the end of the sixteenth century. Thus, we are faced with a contradiction that we already know about regarding the chronology of the sixteenth-century economy: the century of Dutch

Table 7.26 Cultivation in East Frisia, 1633–1794

Years	*Area*
1633–60	6,043 diemath
1661–1735	0
1736–94	8,139

Note: (1 diemath=0.9822 hectare)

agricultural expansion (*c.* 1590–1670) contrasts with the agricultural sixteenth century of the rest of Europe (about the end of the fifteenth century to the end of the sixteenth century). It seems to me that this discrepancy is important, and I shall return to it later.

Thus we have three examples, Italy, Poland and Holland, two of which accord while the third is an exception. The cases of Italy and Poland are confirmed by those of France,[120] Spain[121] and Germany,[122] while that of Holland corresponds, to some extent, with England.[123]

I realize that it is easy to find exceptions to the rule in the agricultural sector. A corner of a province, the accounts of a particular estate, the development of a certain enterprise can have their own, exceptional characteristics. Neither is the seigneurial reaction manifest in the same way in all countries. For example, in Italy money was invested in official acquisitions (which conferred a certain 'official' status on to some domains); in Poland the nobility sought to compensate for the decrease in cereal production by distilling, so they used grain to create an industrial monopoly on beer and brandy But the net result was the same in most places at this time: agricultural production declined, or at best stagnated. The growing 'feudalization', evident in the collection of revenues, was sterile in itself and sterilizing in its consequences. It produced a structural economic stagnation.

Turning away from the particular characteristics and postponing our study of Holland and England, we shall try to make some observations that may be applied to Europe as a whole.

First of all, it may be asked how it is that trade or industry develops in an economic sphere, where the majority of inhabitants do not participate in the life of the commercial market? The two shillings that a peasant may spend at the market during a prosperous period go to maintain, directly or indirectly, much of international trade. When he does not spend the two shillings, who will buy the spices and cloth imported? The lord? The latter is, in spite of his wishes, not able to proceed beyond a limited consumption of spices and cloth. *Luxus und Kapitalismus*, the fine formula of Werner Sombart, is valid mainly for the Middle Ages, but should be revised seriously if the economic historians of the modern world would have a similar vision: it is useless to look for commercial or industrial development when agriculture is faring badly. In these centuries before the nineteenth, trade and industry prospered during the times when agriculture retained its vitality. This was not solely because the peasants were the direct

consumers, but because they promoted (indirectly) a class of consumers in the cities which outnumbered the lord and his entourage.

We should reflect on these elements before embarking on explanations that cannot be proved, other than by arbitrary illustrations. It is an illusion to think that there were completely autonomous fluctuations in trade and industry before the nineteenth century. The great phases (positive and negative) of commercial life (and of industry) responded, in these societies of markedly agricultural character, to the significant rhythms of agrarian life. When agriculture goes, everything goes, for want of a better phrase.

To return to our theme, after the end of the sixteenth century European agriculture showed definite signs of exhaustion. Refeudalization began to make its effects felt, and the agrarian structure of almost all Europe was weighed down by the strain. This is shown not only in the qualitative data of production, but also in other sectors beyond the purely economic. The phenomenon was general. Trade and industry managed to circumvent this weakening of the agrarian structure, overcoming the pre-crisis of 1609–13 and forging ahead again. They had, however, recourse to a particular 'stimulant': credit. I shall return to this aspect in the second section of this article. In spite of every 'stimulant', trade and industry did not manage to pass through the major crisis of 1619–22. Originally a normal, cyclical crisis, it was transformed into a structural crisis because the support of agriculture was no longer there. If, at the back of Seville, or Danzig or Florence or Lübeck, there had been a healthy agrarian economy, this crisis of 1619–22 would have remained a (more or less) minor event. Is this an oversimplification? How else are we to explain such significant exceptions, discordances and agreements?

By examining the great trades and industrial production as a whole, we have seen that the depression in these economic sectors was not so drastic in Holland and England as in other regions of Europe. Perhaps, instead of a depression, it should be described as a stagnation, or merely as a decrease in vitality. We shall now return to the conclusions previously advanced for the case of Holland. Basically, I put forward the idea of a 'Dutch agricultural century' (*c.* 1590–1670) and placed this against the wider agricultural sixteenth century of Europe (the end of the fifteenth century to the end of the sixteenth century). I do not know how far this idea will be accepted, but either way, what seems to me important to emphasize is that many of the Dutch trade regulations coincide perfectly with the agrarian chronology that I have suggested. For example, the number of ships engaged in the trade of the East India Company is shown in Table 7.27.[124]

It is not difficult to observe in this different chronological arrangement the affinity of movement between the series of figures and those previously cited for land reclamation. Does not the reduction in the number of ships between 1671 and 1701, at least to a great extent, coincide with the period of

Table 7.27 Number of ships engaged in the trade of the Dutch East India Company

Years	*Number of ships*	*Years*	*Number of ships*
1611/12–1620/1	117	1701/2–1710/1	271
1621/2–1630/1	148	1711/2–1720/1	327
1631/2–1640/1	151	1721/2–1730/1	379
1641/2–1650/1	162	1731/2–1740/1	365
1651/2–1660/1	226	1741/2–1750/1	315
1661/2–1670/1	257	1751/2–1760/1	276
1671/2–1680/1	219	1761/2–1770/1	303
1681/2–1690/1	209	1771/2–1780/1	294
1691/2–1700/1	241	—	—

contraction in land reclamation? Is this a coincidence? If it is not a mere coincidence, we may surmise either that it was the decline in trade with the East Indies that decreased agricultural production, or, more accurately, that it was the stagnation of agricultural life that brought about the decline in trade.

Holland and England are, however, exceptional regarding the absence of a massive and unequivocal 'turning-point' that seemed to affect all other places. They are the exceptions that, so to speak, prove the rule. It seems that Holland and England were the only two countries that escaped the phenomenon of refeudalization. Holland and England, the former more than the latter, continued throughout the seventeenth century to maintain an agricultural life of a higher level than the rest of Europe. We may recall, for instance, the curious phenomenon in Holland of the 'tulip mania'.[125] This passion (which incidentally caught the attention of the rest of Europe, because tulips, bought by the bag, grew into beautiful flowers) could never have occurred in countries such as Italy, or Spain, or Poland or Germany. Was this an isolated example of agrarian enterprise? But then, how are we to explain that, while all Europe was struggling with agricultural decline Holland maintained an equilibrium? Certainly, not everything concerning agriculture in Holland and England was idyllic, but the fact remains that these two countries continued throughout the seventeenth century to invest large amounts of money in land. It was this that not only gave vitality to the existing economic life, but established the preconditions for the agricultural and industrial revolutions that followed.

Summary

We are now able to draw up a sort of balance sheet of the characteristics of the two centuries, which we have reviewed somewhat hastily.

1 The sixteenth century, a century that begins at the end of the fifteenth century and ends between 1600 and 1620, was marked in Europe (and

beyond Europe) by a general economic development, brought about by an expansion in agriculture, which also allowed the boom in trade and industry.

2 After 1600, the commercial and industrial sectors, lacking the support of agriculture, were maintained for another twenty years, to lose their driving force only after 1620.

3 The seventeenth century seems to have been characterized by a stagnation in economic life. Holland escaped from this and so, to some extent, did England.

The reader may well be surprised that I should attribute so much to the effects of the crisis of 1619–22. He may say: how is it possible that even so important a crisis could shake the foundations of industrial production and commercial distribution previously so secure and extensive? What happened to the investments of businessmen and merchants? The answer is simple. It is necessary to remember that in the industrial and commercial sectors of the world, until the eighteenth century, the proportion of fixed capital was minimal in relation to that of floating capital; consequently, it was very easy to pull out after a round of business affairs bearing real capital.

In addition to this, I should like to point out that in the sixteenth century the old Mediterranean economies, such as that of Florence, were expanding, as were those of England, France and Holland. In the seventeenth century, however, the Dutch and English economies stood alone (and they did not abandon their old rivalry). Thus, the seventeenth century was for all Europe, indisputably, a time of regression.

We may conclude that the sixteenth century tried to break out of the web of a 'medieval economy', to enter a 'modern economy'. The resistance was too strong, and the old forms of production, and the old relationships in production, prevailed. The 'capitalist' experiment of the sixteenth century ended in the return of the feudal type of economy. It was the eighteenth century that allowed the definitive departure of a good number of European regions from these old economic systems, thus resuming the movement that failed in the sixteenth century.

II
THE INTERNAL MECHANISM OF THE CRISIS

We must now move from the long term to the short term and try to identify the hinge that joins together the two long phases of the sixteenth and seventeenth centuries. It should not have escaped the reader's notice that, in the first part of this chapter, there was no mention of money, prices (there was a very rapid allusion to these), exchange or banking. These matters, which I now wish to discuss, were essentially facts of production and distribution, not facts *connected* with production and distribution. If prices fall, and if monetary issues show a contraction, this is because production has contracted, and this contraction

obviously cannot be attributed to the fall in prices. This principle is indisputably correct as regards long-term trends, but with the short term in view, it may fairly be said that prices, money, banking and exchange are all factors that have their own value, as far as trends are concerned. These must, therefore, be considered.

We shall now turn to money and the metals used for money. There is an abundant supply of material at our disposal. But before entering into the details of specific situations, I think that it is important to note the relationships between the types of precious metals. Basically, after 1620, it was the reserves of gold that could dictate events.[126] As a general rule, each surplus of wealth tended to transform its capital, or the profits created by it, into gold, thus hoarding wealth and making it sterile. There was some attempt to counteract this by the introduction of small change and of copper in particular, but this was doomed to failure.[127]

We shall now embark on a more detailed examination of the monetary situation in the period 1613–22. I believe we may look at this from two viewpoints, one being the totals of money issued, and the other being the relationship that existed between credit and real money. This last aspect concerns a phenomenon common without exception to all Europe: the progressive increase in the value of metal coinage in terms of money of account. This is not a new factor, peculiar to the period we are now studying. What seems to me important is that, while this evaluation continued regularly, and sharply, after 1619, all European money, without exception, underwent a drastic devaluation.[128] I stress: what was important was the *intensity* of the phenomenon, together with its epidemic character, and the severe aggravation of the problem clearly shown in the years 1619–22.

The coinage figures are, obviously, less clearly characterized. In their case there are a number of external factors—the reissue of old money, issues using old stocks of metals, issues to cover the overspending of the State—that obscure or alter the real situation of the money market. However, I think it is possible to maintain that:

1 In the case of France, during the period 1608–35 (with the sole exception of the year 1615), the issues of money show a considerable reduction. From this vantage point, it is possible to say that the crisis of 1609–13 was never resolved. The minimum issue was reached in 1622, with £163,236 tournois; this was preceded by another low point of £165,785 in 1620. If the totals for the decades between 1561 and 1640 are studied, these minima assume a greater significance (see Table 7.28).[129] Leaving aside the inflationary figures of the last decade (they are inflationary owing to the reissue of old money), it is evident that there is a steady contraction in issues of money which begins in the last decade of the sixteenth century.

Table 7.28 Coinage issued in France, 1561–1640

Years	*Coinage issued (£)*	*Years*	*Coinage issued (£)*
1561–70	12,633,438	1601–10	14,527,552
1571–80	18,832,520	1611–20	7,351,539
1581–90	29,737,772	1621–30	2,888,829
1591–1600	11,896,571	1631–40	30,103,704

The two minimum issues are significant in so far as they occur in the context of a progressively falling monetary stock.

2 In the case of Milan, rather than citing figures I shall quote C.M.Cipolla:

> Without much searching, we see, in the level of coinage issues, the occurrence of the 1607–9 crisis, that very acute crisis of 1619–22, and again that of 1686. The extent of the fluctuations corresponds surprisingly well to the movements of the conjuncture. During the 1619– 22 crisis, the total issue falls from 4,770,000 *lire imperiale* to 250,000. If, moreover, we take into consideration the fact that these figures should be set within the context of a deflationary movement of the exchange rate, and therefore that the real value of these issues was far in excess of 4,770,000 lire at the optimum point of the boom, it follows that the crisis of 1619–22 saw a contraction of issues at least equal to 97 per cent. Further, during the crisis of 1619–22, just as during that of 1686 (and later), it is obvious that the contraction only touched the issue of *monete grossi* [gold and silver coins], while that of *monete piccoli* [copper coins] followed an inverse path.[130]

Everything appears more clearly if, as Cipolla has done, all the period 1580–1630 is taken into consideration.[131] Over these years, despite the generally high level of issues between 1590 and 1605, there is a boom which is followed by a stagnation.

3 The situation in Lisbon shows a downward trend between 1609 and 1615; from this date until 1619 there is a recovery that is overwhelmed by a violent fall. This reaches its nadir in 1622, after which there is a recovery until 1626, which in turn is followed by another violent fall, lasting until 1634.[132] Unfortunately, data at our disposal do not go beyond 1604, but F. Mauro, a specialist in Portuguese economic history during the sixteenth and seventeenth centuries, writes of 1604–40 as a ‘période de grand stabilité monétaire’.[133] However, this period of great stability was also characterized by violent oscillations. The stability is achieved only when the maxima and minima have been averaged out, and their own polarities discarded!

4 The case of London is very similar to that of Portugal.[134] There is a limitation of issues between 1609 and 1612; a recovery until 1617, a fall

> until 1620, a recovery until 1627, a stagnation until 1636. The oscillations here are, however, much less pronounced than they were in the case of Lisbon; there is notable stability, which I should like to call a stagnation, in the issues of money (apart, that is, from the important contraction of 1619–20).

What conclusions may we draw from these four cases? I believe it is possible to maintain that the first forty years of the century showed the signs of a steady, or sometimes direct, contraction in the issue of money. This is reinforced by E.J.Hamilton's work on the arrival of gold and silver in Seville: the maximum was reached between 1591 and 1600, to be followed by a steady diminuition.[135] The intense and sudden fall of the years 1619–22 takes place against the background of this chronic contraction.

I believe that what I have said so far will arouse little opposition. The crisis of 1619–22 has often been noted in the sphere of monetary policy, ever since the vintage study of W.A.Shaw.[136] Perhaps because of this, English (and also Polish) historians have found it easy to believe that the origin of the crisis was monetary, which is like looking at a shadow rather than at reality. We ought to remember that, as regards money, the years 1613–25 were years of insufficient monetary issues and that between 1619 and 1622 a severe drop in monetary issues was experienced everywhere. However, we cannot further elaborate on this for the data are not available.

There is an anomaly that may be discerned at once. If monetary issues contracted, if during the years 1619–22 the nadir was reached, how are we to explain the fact that:

> in Genoa, at the beginning of the seventeenth century [that is, in its first twenty years], there was regular lending of capital at one and two per cent. It was the first time in the history of Western Europe, since the fall of the Roman Empire, that capital was offered so cheaply, and this constituted a major revolution.[137]

How do we reconcile the fact that in 1619 money was offered at 1 and 2 per cent, the absolute minimum interest rate throughout the period 1522–1625?[138] On the one hand we have a very low rate of interest, while on the other we have a reduction in the issues of money. The contradiction seems flagrant and insoluble. In fact, I believe that a solution can be found in at least two ways.

The first concerns the banking situation. I believe that there are few periods so extraordinary in the history of banking as that of 1613–22. We may follow the case of banks in Naples, Siena, Strasbourg and Colmar in order to obtain a fair geographical and economic representation. Although they are in different zones, they show the same characteristics.

In the case of Naples, we may observe two banks: the Banco dello Spirito Sancto (from 1591 to 1664) and the Banco dei Poveri (from 1605 to 1624).[139]

Table 7.29 Percentage increase of the fiduciary circulation of the Banco dei Poveri

Year	*Increase %*	*Year*	*Increase %*
1607	1,109.75	1617	9.08
1608	98.14	1618	21.94
1610	270.83	1619	5.36
1611	13.67	1620	6.51
1612	24.93	1621	80.11
1613	19.34	1622	47.01
1614	20.98	1623	66.87
1615	14.77	1624	56.56
1616	27.54	—	—

Table 7.30 Financial transactions of the Banco dello Spirito Sancto, 1613–23

Year	*Total assets (lire)*	*Cash*		*Investments*		*Loans*	
(lire)	*%*	*(lire)*	*%*	*(lire)*	*%*		
1613	594, 166	131,116	22.0	172,106	28.9	290,945	50.1
1614	732,516	112,628	15.3	251,850	34.2	368,038	51.5
1615	802,266	29,978	3.7	257,328	32.2	514,960	64.1
1616	1,147,979	67,425	5.9	422,543	36.8	658,011	57.3
1617	924,931	118,057	12.7	447,220	48.4	359,654	38.9
1618	872,599	120,242	13.7	362,613	41.5	389,744	44.8
1619	829,400	71,010	8.5	309,327	37.3	449,063	54.2
1620	946,785	57,846	6.1	332,015	35.1	556,924	58.8
1621	1,132,262	225,187	19.8	366,385	32.5	540,690	47.7
1622[1]	1,420,996	517,790	43.4	356,412	25.0	546,794	31.6
1622[2]	1,392,463	151,969	10.9	353,412	23.3	887,082	63.8
1622[3]	1,245,946	51,249	4.1	353,412	28.3	841,825	67.6
1622[4]	1,100,139	35,473	3.2	285,460	25.9	779,206	70.9
1623[5]	1,020,463	20,453	2.0	286,826	28.1	713,184	69.9
1623[6]	985,233	17,947	1.7	286,820	29.0	681,466	69.3

Notes: 1 To 28 February
2 To 10 June
3 To 30 September
4 To 24 December
5 To 10 April
6 To 30 July
All other dates are to 24 December

What is at first striking is that in both cases there is a clear manifestation of inflation of credit during the period 1610–22, above all in 1615. In the case of the Banco dei Poveri, the percentage increase on the fiduciary circulation from

one year to the next is shown in Table 7.29.[140] For the Banco dello Spirito Sancto, there are other factors that show similar trends and point to the same conclusions as the previous figures (Table 7.30).[141] The collapse appears here, as before, very clearly in 1622. There were, however, warnings spread abroad as early as 1617, as is shown by the order, 'all the debtors of the bank will pay what they owe'.[142] But this, and other demands, had no effect, and matters continued until their inexorable climax in 1622.

In Siena between 1570 and 1593 the Banco Pio, or 'Lombard', closed seven balance sheets with a deficit during the period of nearly twenty-four years, while between 1594 and 1624, over a period of thirty-one years, only one balance sheet was closed with a deficit (in 1622).[143] In addition to this, from 1596 the reserves had been rising steadily. This increase was not, however, an expression of healthy economic life. We know that the Pio Monte was engaged in the practice of making long-term loans to people who, in their turn, invested this money in exchange, so that in 1619 an order was given that, 'creditors may not continue to hold any kind of letter of credit for more than a period of three years'. This had the effect of calling money back into the reserves, but at the same time it created 'shortage of money in the city of Siena'.[144] We may conclude that the development of the Banco Pio, which was a *monte di pietà* (a charitable institution which advanced money but demanded pawns or pledges in return), was governed by speculative pressures and the fluctations of the credit market.

The Municipal Bank of Strasbourg[145] shows a series of big profits between 1591 and 1622, while the years 1623 and 1624 (especially the latter) brought few profits. After this last year, until 1670 there was a general contraction of profits. The Bank of Colmar seems to follow the same trend. It too saw the extraordinary development of credit, especially between 1614 and 1617, when there was a notable expansion of *créances actives*.[146]

To complete our investigation into European banking, we shall look at Amsterdam and Middelburg. Here, more than anywhere else, the speculative character of banking is evident. The dates of the foundations are, in themselves, significant: Amsterdam in 1610, Middelburg in 1616. In both cases, what is striking is the enormous increase in the number of holders of credit accounts (Table 7.31).[147,148] This can be interpreted in a numberof ways. What seems to me significant is the relationship between credit and metal reserves. In Amsterdam a strict balance was observed until 1619, after which there was a substantial divergence.[149] The same is true of Middelburg. Following a year of equilibrium, there was a dangerous distortion in the relationships of figures for 'credit' and those for metal reserves. The fact was, a euphoric atmosphere had arisen regarding credit, as is testified by the following interesting document (there are others like it):

> The Burgermaster and the Council are concerned that for some time, letters of exchange (obligations and assignations) have been used in Middelburg, to vouch for debts, instead of paying them directly in cash, and that the

> letters do not pass through the hands of just two people, but sometimes seven or eight, and furthermore, they all pay one and the other, with the same document.[150]

Table 7.31 Number of holders of credit accounts in Amsterdam and Middelburg, 1611–31

Year	*Amsterdam*	*Middelburg*
1611	703	—
1617	—	484
1620	1,207	—
1627	1,312	—
1631	1,348	564

There are many other symptoms of the inordinate increase in credit. The rising numbers of agents of exchange, the fact that the municipality of Amsterdam began to lend money at the end of 1615 to the East India Company, despite all the prohibitions and all the appeals of the authorities—all these suggest that even the cities of Holland did not escape the malaise of the time.

Another way of comprehending the apparent contradiction in the low price of money and its falling quantity lies in the observation of the movement of exchange rates in many European cities based on the fairs of 'Bisenzone' held at Piacenza. I myself have studied this, in collaboration with my colleague, José Gentil da Silva, and together we arrived at the following conclusion. Between 1600 and 1619 the machinery of credit and exchange was characterized by an extraordinary amount of speculation.[151] After this last date, the trend in the fairs is such that we have referred to it as an *'embourgeoisement'*. How could this conclusion be avoided, after looking at graph 3 in that article, which shows that after 1619 profits were becoming smaller but, above all, more regular! Less profit was made, but less was at risk. Until 1620 the risk was considerable (relatively speaking, since the financiers were careful), but the potential profits great. After the collapse in 1620 there was a sharp recession.

How much of what we have said so far regarding money, interest and credit tallies? First of all, we may summarize the conclusions as follows:

1 A stagnation in the minting of money resulted in a reduction in the monetary stock.
2 The price of money was progressively falling.
3 There was a very considerable expansion in credit.

These three conclusions, between them, show a clear contrast (2 against 1), and an agreement (3 and 1) that explains the contradiction. It seems possible to conclude that the increase of credit, brought about by the diminution of metal reserves, not only compensated for this diminution but went beyond this, precipitating a notable reduction in the price of money.

It would be tedious to search for the exact date of the first exchange operation. It seems certain that the practice began to expand and become more common only during the last decade of the sixteenth century. A Venetian document of 14 December 1593 indicates this clearly:

> This practice has been in existence for *a certain time* [my italics] by which, instead of making payment at the bank, or in ready money, they have reduced the credit of an individual on to paper, and they pass this around from hand to hand.[152]

An odd document this, which more or less unconsciously echoes the Dutch text included on p. 193 above. 'Giros' and 'assignations' were the means which businessmen used—indeed, had adopted—to cope with the difficulties caused by the lack of precious metals. They were innovations that required a high degree of manipulative skill on the part of the businessmen, whose inadequacy bears some resemblance to that of the sorcerer's apprentice. They believed that they could control credit through monetary manoeuvres, and in fact this did solve, at least partially, the crisis of 1609–13, but it was to have no effect during 1619–22. The new stimulant was miraculous, for a little more than twenty years before its efficacy dwindled. The inflation of credit passed to monetary inflation and to the contraction of all forms of credit.

Were I an economic historian concerned, above all, with 'metals', I should stop here. I should, perhaps, emphasize the failure of credit without sufficient support of metal reserves (as if credit would be employed when the stock of metal was increasing); and the inadequacy of the mental structure of the businessman faced with an innovation such as credit (and there is certainly some truth in this); and then I should close. But I have already stated that I am firmly convinced that the crisis of 1619–22 (a normal, cyclical crisis) occurred during a structural crisis, which for some time had had its effect on the world of agriculture. It will be necessary to try to approach an understanding of this trend in the short-term period.

However, we shall begin with the long term, taking the prices of agricultural products. The decade 1611–20 is characterized in all Europe by a fall in agricultural prices, which contrasts with the sustained price level of prices of industrial products. This reduction in agricultural prices (they had been decreasing steadily since the decade 1591–1600)[153] was disastrous for the peasant world, more so because the harvest yields had stagnated for some years, at least from the end of the sixteenth century.[154] Thus, the possibilities of saving and spending were reduced. From 1618 agricultural prices rose and reached their maximum in 1622.

The equilibrium (artificial in fact) that had been maintained during the previous years was broken between 1619 and 1620. But the rise in prices did not represent an increase in the value of agricultural products: it was merely an expression of poor harvests. Landlords and peasants were selling smaller

quantities at higher prices, and consequently they did not manage to ameliorate the situation. On the contrary, the high prices of agricultural products made it necessary either to raise wages in the urban areas or to make largescale dismissals.[155]

The mechanism of the crisis was as follows. The prices of industrial products fell because producers and merchants who utilized the 'giro' system were obliged to comply with the demands of the banks; the latter, in difficulty with contracting metal deposits, could not, as they had before, allow unlimited time for repayments. In addition to this, the price rise in food products forced consumers to concentrate their purchasing power in that direction, thus sacrificing industrial products, and bringing about their decline. To complete the picture, the men in government, and public opinion in general, blamed all these calamities on the monetary situation, thus forcing the government to initiate a drastic deflationary policy, which did not succeed in revitalizing the business world. The other, more important, crisis, in agriculture, was overlooked.

We shall now take a particular example. We have seen from the tables cited that the Italian commercial and industrial life expands, or at least is very stable, during the course of the second decade of the seventeenth century. In a recent study[156] of the accounts of an agency of an estate in Lombardy, it is evident that, between 1600 and 1647, there was a progressive decrease in profits, which culminated in a deficit during the last seven years. But I would sooner give the complete figures (Table 7.32).[157] I have produced the table in

Table 7.32 Percentage increase or decrease in accounts of an agency of an estate in Lombardy, 1600–47

Years	*Increase %*	*Years*	*Increase or Decrease %*
1600–07	+35.92	1624–31	+6.37
1608–15	+31.84	1632–9	+2.37
1616–3	+28.57	1640–7	−5.07

full, not merely to give greater force to my argument, but to avoid drawing false conclusions about the seventeenth century The negative period 1640–7 occurred within the context of a decline that was already under way by the beginning of the century (unfortunately, these figures date only from 1600). It is against this background of contraction that the first crisis of 1609–13 and the later crisis of 1619–22 occurred. One event reacted upon another. The structural crisis of agriculture made the cyclical crises more serious. These, in their turn, aggravated the weakening agrarian structure, which affected the succeeding circumstantial crisis.

I repeat: what was important was the effect of the agricultural decline upon the crisis of 1609–13, a crisis of circumstance. The latter weakened the agrarian structure, which in turn affected the crisis of 1619–22.

The reader will excuse the polemical tone—somewhat excited—in the latter part of this chapter. Apart from the fact that I personally consider zeal to be one of the essential qualities of the historian, I wanted not merely to relate the extrinsic events of a crisis, but to expound its characteristics and its nature; and further than this, to rediscover the spirit of the two centuries, one that preceded the crisis, the other that followed it. The crucial points of contrast are the agrarian depression of the seventeenth century (which began in the last decade of the sixteenth century) and the notable conquests of the sixteenth century. These last, the fruits of ambition, and a perhaps excessive enthusiasm, failed, and the stultifying agrarian depression lumbered heavily to the fore, to characterize a long period of economic history.

POSTSCRIPT (1996)

Naturally I am flattered that my article is being republished, but I should issue a warning to readers. When I published the original version, in 1962,1 intended to compare and contrast the situations in Europe and Latin America. However, I had scarcely begun my research on the latter and so I fell into an error that has claimed many other scholars: I assumed that certain Spanish (or Portuguese) 'facts' provided reliable evidence about conditions in America. I thought, for example, that the statistics on Spain's transatlantic trade constituted a useful index of the colonies' economic fluctuations. Of course I was wrong. The data patiently collected by Pierre and Huguette Chaunu, and by García Fuentes, certainly reflect the problems of the Spanish economy, but they tell us nothing about America. If Spain's transatlantic commerce declined, it demonstrates only the weakness of the Spanish economy; by contrast, the colonies' capacity to absorb European merchandise grew steadily, and the English, the Dutch and the French —albeit in massive and sustained violation of Spain's alleged monopoly—made up for the fall in Spanish export goods.

In the same way, I concluded from the Spanish government's own records that the production of precious metals fell during the eighteenth century, whereas in reality it soared to unprecedented heights. In my defence, I did not then have to hand the excellent study by Michel Morineau, which reconstructed more realistic figures from other sources; but I was wrong all the same, and the error remains significant.[158]

To take another example: when I wrote my article, data on price movements in Latin America were scarce, but they seemed to suggest a downward trend during the seventeenth century. However, subsequent studies have clearly shown that American prices rose in this period, in stark contrast to the stagnation or fall of European prices. In short, Latin America appears to have followed the opposite economic trajectory to that of Europe.

Readers need not fret: I do not seek to hide my shortcomings in the bibliographic lacunae of the past. I repeat that I suffered from a sort of naïveté common among European historians, imagining that I could extrapolate

from the Iberian colonial system conclusions that appertained, properly speaking, only to Spain and Portugal (or perhaps to all of Europe, except for Britain and the Dutch Republic). In my defence, I can say that I was not the only one to make this mistake—even veteran Americanists (from both sides of the Atlantic) have shared the same naïve vision—and I have tried to redeem my errors in a recent book whose title reveals its argument: that Europe and Latin America developed in opposite directions.[159] As far as Europe is concerned, however, I believe that the evidence and the conclusions in my chapter will still pass muster.

I would like to make one further comment. For some time now, a veritable torrent of historical works has suggested that the study of 'classic' historical topics is worthless; that, for example, the study of crises is old-fashioned. This I fail to understand. Surely the 'new history' cannot entirely ignore crises and cycles? The fact that the subject has been studied before—going back to the seven lean years followed by the seven fat years of the Old Testament— does not necessarily deprive it of interest. I belong to the generation of Marian Malowist, Eric Hobsbawm, Hugh Trevor-Roper, Wilhelm Abel, Bernard Slicher van Bath and countless others who have studied, since the 1950s, often from totally different historiographical positions, the crises that afflicted Europe between the fourteenth and the eighteenth centuries; yet I have never believed (nor do I think any of those named above believe) that the subject is a new invention. One can find the idea of 'crisis' in the writings of eighteenth-century historians, concerning both seventeenth-century Spain and the Roman Empire. How could one think something is new when it can be found in Gibbon? All I and the others have done is to enrich the concept with insights developed by economists since the nineteenth century, or to incorporate the experience of the (short-term) crisis of 1929–32. This refinement of the original idea is important; but it is not an invention.

Finally, long-term, structural crises have indeed occurred in the past, and we may well be experiencing one now.[160] I therefore believe that past crises still deserve study because they are still relevant; and I invite the reader to view my text as a modest proof of History's continuing concern to illuminate the world in which we live.

NOTES

* Translated by Margaret Wallis. First published in Italian in *Rivista storica italiana*, LXXIV (1962), pp. 480–531. The article summarizes a series of lectures given during the academic year 1960–1 at the Ecole Pratique des Hautes Etudes, VIe Section (Sorbonne). In addition to this, it draws on a previous article, 'A Florence, au XVIIe siècle: industries textiles et conjoncture', *Annales E.S.C.*, VII (1952), pp. 508–12. I should like to thank my friend B.Geremek for assisting me with the editing and also for his criticisms.

1 'The seventeenth century, the last phase of the general transition from a feudal to a capitalist economy', was the judgement of E.J.Hobsbawm, 'The general crisis of the European economy in the seventeenth century', *Past and Present,* V and VI (1954), p. 33. This article seems to me fundamental in its precise assessment of the seventeenth century. It is, however, necessary to point out that, if the seventeenth century represented the last phase in the transition from a feudal to a capitalist economy, the sixteenth century contained within itself many 'liberating' elements. It seems to me that the significance of the seventeenth century lies in the fact that it was, on the one hand, a preparation for the transition to a capitalist economy, as Hobsbawm shows, and that, on the other, it witnessed a feudal reaction (at least during the earlier years) to those elements of capitalism (a capitalism basically mercantilist) that had already developed in the sixteenth century. In the same journal, *Past and Present,* XVI (1959), there is an article by H.Trevor-Roper, 'The general crisis of the seventeenth century'. See also the various criticisms of that article in *Past and Present,* XVIII (1960).

2 F.Baehrel, *Une Croissance: la Basse-Provence rurale (fin du XVIe siècle—1789)* (Paris, 1961), p. 57.

3 For this, see the introduction to the collection of works that I have compiled under the title *Storia dei Prezzi: Metali, Risultati, Problemi,* to be published by Einandi.

4 H. and P.Chaunu, *Séville et l'Atlantique (1504–1650),* 7 vols (Paris, 1955–7) and 1 vol. of 'construction graphique' (Paris, 1957).

5 N.Ellinger Bang, *Tabeller over Skibsfahrt og Varentransport gennem Oresund,* vols I and III, for the period in which we are interested (Copenhagen and Leipzig, 1906 and 1923, respectively).

6 In the first place, A.E.Christensen, *Dutch Trade to the Baltic about 1600* (Copenhagen, 1941); P.Jeannin, 'Le tonnage des navires utilisés dans la Baltique de 1550 à 1660 d'après les sources prussiennes', in *Le Navire et l'économie maritime. Travaux du troisième colloque d'histoire maritime* (Paris, 1960); A.Friis, 'Le valeur documentaire des comptes de péage du Sund. La période 1571 a 1618', in *Les Sources de l'histoire maritime du Moyen Age au XVIIIe siècle, articles présentés par M.Mollat* (Paris, 1962); P.Jeannin, 'La conjoncture commerciale à la fin du XVIe siècle et au début du XVIIe siècle: ce que donnent les comptes du Sund', in *XIe Congrès international des sciences historiques. Résumés de communications* (Stockholm, 1960); P.Jeannin, 'L'activité du port de Koenigsberg dans la seconde moitié du XVI siècle', *Bulletin de la Société d'Histoire Moderne,* LVII (1958).

7 H. and P.Chaunu, *Séville et l'Atlantique,* VI, pt.1, p. 341.

8 Ibid., p. 474.

9 P.Chaunu, *Séville et l'Atlantique. Structures et conjoncture de l'Atlantique espagnol,* 3 vols (Paris, 1959).

10 Ibid., vol. 3, pt 2, 2, p. 1529.

11 Ibid., p. 1132.

12 Ellinger Bang, op. cit., I, p.x.

13 Christensen, op. cit., p. 313.

14 Ibid., pp. 444–5.

15 Ibid., pp. 446–7. For the analogy with English trade passing through the Sound, see the reconstruction of R.W.K.Hinton, *The Eastland Trade and the Common Weal in the Seventeenth Century* (Cambridge, 1959), pp. 227–9.

16 Christensen, op. cit., p. 103, n. 1.
17 Ibid., p. 465.
18 It is easy to arrive at these conclusions on the evidence of graph XVI (grain) of the work of Christensen. For salt, I have had recourse to a graph constructed by my friend Pierre Jeannin, who allowed me to use it and whom I should like to thank. Other graphs, which are part of the work that P.Jeannin has been compiling for a long time on Baltic trade during the sixteenth and seventeenth centuries, show a similar fall for rye, pitch, charcoal and tar. The movement of the three latter products is especially significant since they hardly lend themselves to involvement in smuggling—the tariffs they carried were so small.
19 1 await the volumes on the movements at the port of Cadiz, which H. and P. Chaunu have promised, and which should certainly equal the excellence of those that describe Seville. Meanwhile, see A.Girard, *La Rivalité commerciale et maritime entre Séville et Cadiz jusqu' à la fin du XVIIIe siècle* (Paris, 1932). [But see also Steensgaard's comments on pp. 34–40 above—eds.]
20 K.Heeringa, *Bronnen tot de Geschiedenis van den Levantschen Handel*, II (The Hague, 1910), pp. 1042ss., n. 451. And also, H.Wätjen, *Die Niederländer im Mittelmeergebiet* (Berlin, 1909)
21 See F.Braudel and R.Romano, *Navires et marchandises à l'entrée du Port de Livourne, 1547–1611* (Paris, 1951), pp. 49–55.
22 In the Danzig archive, 300/LIII/147, see the letters of 1590, 1591, 1592, which Florence and Venice gave their merchants, authorizing them to buy grain for their cities.
23 It is, however, significant that a document of October 1612 in Heeringa, op. cit., vol.1, p. 436, n. 219, makes allusion to the fact that the Dutch merchants in the Levant had worked there for 'twelve or thirteen years'.
24 Ibid., p. 424, n. 213.
25 H.Brugmans, *Geschiedenis van Amsterdam*, III (Amsterdam, 1930), p. 30.
26 Heeringa, op. cit., I, p. 432, n. 216.
27 Ibid., p. 429, n. 215.
28 Ibid., p. 195, n. 127.
29 Ibid., p. 256, n. 132.
30 Ibid., p. 486, n. 236, n. 6.
31 Ibid., p. 485, n. 234.
32 Ibid., p. 494, n. 240.
33 Ibid., p. 497, n. 241.
34 Ibid., p. 514, n. 246.
35 Ibid., p. 525, n. 249.
36 Ibid., II, pp. 802–3, n. 361 and pp. 988–9, n. 426. France also suffered a recession of trade in the Levant, as is shown by P.Masson, *Histoire du commerce français dans le Levant au XVIIe siècle* (Paris, 1896), pp. 118–30.
37 Heeringa, op. cit., vol.1, pp. 53–5, n. 31.
38 Ibid., pp. 58–9, n. 35.
39 Ibid., pp. 61ff., n. 40; and also Brugmans, op. cit., III, p. 166.
40 Ibid., p. 72, n. 55.
41 For the problem of the English presence in the Mediterranean, see R.Davis, 'England and the Mediterranean 1570–1670', in F.J.Fisher (ed.), *Essays in the Economic and Social History of Tudor and Stuart England* (Cambridge, 1961).

42 The figures are taken from *Bilanci Generali della Republica di Venezia,* I, i (Venice, 1912), pp. 284 (for 1582), 365–6 (for the years 1587, 1594, 1602), 471–2 (for 1621), 487–9 (for 1633), 563–4 (for 1640). For the significance and details of the exaction of customs duties, see the introduction to the above-mentioned volume.

43 See F.C.Lane, 'La marine marchande et le trafic maritime de Venise à travers les siècles' in *Les Sources de l'Histoire maritime en Europe, du Moyen Age au XVIIIe siècle, articles présentés par M.Mollat* (Paris, 1962), pp. 28–9. For other aspects of marine economy (above all, the construction of ships), see the next volume for the report on 'La marine marchande vénitienne au XVIe siècle', particularly pp. 51–5.

44 F.Borlandi, *Il Problema delle comunicazioni nei suoi rapporti col Risorgimento Italiano* (Pavia, 1932), p. 44.

45 See L.Bergasse and G.Rambert, *Histoire du commerce de Marseille,* IV (Paris, 1954), pp. 95 and 189 *à propos* the figures published here, in apparent contradiction to my argument. It should be noted that it was a matter of payments declared by *'fermiers'* and, as Bergasse observes, 'there is no guarantee that the duties levied on the items, which serve as a base from which to calculate prices, remained the same throughout the whole period'. For Livorno [Leghorn], see the excellent article of G.Mori, 'Linee e momenti dello sviluppo della città, del porto e dei traffici di Livorno', *La Regione; rivista dell' Unione Regionale delle Provincie Toscane,* III (1956), n. 12. In addition to this, is it not significant that Livorno, which had originally developed through the grain trade, should have built the greatest number of warehouses for cereals precisely in the second decade of the seventeenth century? (see M.Babuchello, *Livorno e il suo porto* [Livorno, 1932], p. 338). For Ragusa and Spalato see J.Tadic, 'Le commerce en Dalmatie et à Raguse et la décadence économique de Venise au XVIIe siècle', in *Decadenza economica veneziana nel secolo XVII, Atti del Convegno 27 giugno —2 luglio 1957* (Venice and Rome, 1961), *passim,* and particularly p. 263. This volume, with its cross-references to English, French, German and Turkish evidence, supports the argument of this chapter. Everything that has been outlined for the maritime economies is also valid for the 'landlocked' economies, as is shown by B.Caizzi, *Il Comasco sotto il dominio spagnolo* (Como, 1955), pp. 35 and 93–4.

46 C.Biernat and S.Gierszewski, *Statystyka Handlu i Zeglugi Gdańska w XVII i XVIII Wieku* (Warsaw, 1960).

47 S.Hoszowski, 'The Polish-Baltic trade in the 15th–18th centuries', in *Poland at the XIth International Congress of Historical Sciences* (Warsaw, 1960).

48 Ibid., p. 144.

49 Ibid., p. 153.

50 R.Rybarski, *Handel i Polityka Handlowa Polski w XVI Stuleciu,* II (Warsaw, 1958), p. 3. [On Baltic trade, there is a great deal of material available in English in the excellent study of A.Attman, *The Russian and Polish Markets in International Trade, 1500–1650* (Göteborg, 1973): eds.]

51 A.Szelagowski, *Pieniadz i Przewrót cen w XVI i XVII Wieku w Polsce* (1902), pp. 126–7; I have corrected the figures for 1625 and 1627 from a manuscript in the Czartoryski Library in Cracow (no.390 and no.218). On Danzig trade see M. Foltz, *Geschichte des Danziger Stadthaushalts* (Danzig, 1912).

52 Hinton, op. cit, pp. 227–9.

53 F.J.Fisher, 'London's export trade in the early 17th century', *Economic History Review*, III (1952), p. 153, corrected by B.E.Supple, *Commercial Crisis and Change in England 1600–42* (Cambridge, 1959), p. 258.

54 The difficulty of compiling statistics on trade during the crisis of 1619–22 is shown by the article by J.D.Gould, which, despite its title—'The trade depression of the early 1620's' (*Economic History Review*, VII, 1954)—is confined to industrial and monetary matters.

55 J.C.Westermann, 'Statistische gegevens over den handel van Amsterdam in de 17e eeuw', *Tijdschrift voor Geschiedenis*, LXI (1948).

56 I.J.Brugmans, 'Les sources de l'évolution quantitative du trafic maritime des Pays-Bas (XII-XVIIIe siècles)', in *Les Sources de l'histoire maritime en Europe, du Moyen Age au XVIIIe siècle, articles présentés par M.Mollat* (Paris, 1962).

57 I have put this aside here—I propose to return to it in the second part of the chapter. For the figures for the East India Company, see G.C.Clerk de Reus, *Geschichtlicher Ueberlick der administrativen, rechtlichen und finanzieller Entwicklung der Niederländisch-Ostindischen Compagnie* (Amsterdam, 1894), p. 118 and I.J.Brugmans, 'De Oost-Indische Compagnie en de Welvaart in de Republiek', *Tijdschrift voor Geschiedenis*, LXI (1948).

58 J.A.García, *La Ciudad indiana. Buenos Aires desde 1600 hasta mediados del siglo XVIII* (Buenos Aires, 1955), p. 183 and also A.P.Canabrava, *O Comércio portugues no Rio da Prata (1580–1640)* (Sao Paulo, 1944), p. 88.

59 E.F.S.de Studer, *La Trata de negros en el Rio de la Plata durante el siglo XVIII* (Buenos Aires, 1958), p. 102.

60 P.Chaunu, *Les Philippines et le Pacifique des Ibériques (XVIe, XVIIe, XVIIIe Siècles)* (Paris, 1960).

61 Ibid., p. 250.

62 C.R.Boxer, *The Great Ship from Amacon. Annals of Macao and the Old Japan Trade (1555–1640)* (Lisbon, 1959).

63 P.Chaunu, 'Manille et Macao, face à la conjoncture des XVIe et XVIIe siècles', *Annales E.S.C.*, XVII (1962), p. 3.

64 Ibid., p. 577.

65 Ibid., p. 577.

66 Ibid., p. 578.

67 H.Brugmans, *Geschiedenis van Amsterdam*, III, p. 147, and see also H. Dunlop, *Bronnen tot de Geschiedenis der Oostindische Compagnie in Perzië*, I (Gravenhage, 1930), particularly pp.xxxiii and 19, n. 20.

68 It seems interesting to me that F.Argelati, in his *De Monetis Italiae*, III (Milan, 1750), publishes the *Discorso Secondo* of G.D.Turbolo, in which it is mentioned, for the period 1616–29, that there is a return to seven years of prosperity and seven years of depression: an obvious biblical allusion, but still…

69 Proof of the difficulty of constructing any tables to show, indirectly, the global production of industry in Europe is found in the following discussion. It is well known that alum has an important role in the woollen industry; now we know about the alum production of Civitavecchia and that of Spain, through the work of J.Delumeau and F.Ruiz Martin. But, despite this advance, we should proceed with caution, as we do not know about the alum production of Focea, and this ignorance may jeopardize our conclusions.

70 N.W.Posthumus, *De Geschiedenis van de leidsche Lakenindustrie,* 3 vols (The Hague, 1908–39).
71 Sella, *Commerci e industrie a Venezia nel secolo XVII* (Venice and Rome, 1961).
72 Romano, 'A Florence au XVIIe siècle'.
73 Fisher, op. cit.; Supple, op. cit., p. 258; Hinton, op. cit., pp. 227–9.
74 Chaunu, op. cit., VIII.ii, 2, pp. 1974–5; G. Lohmann Villena, *Las Minas de Huancavelica* (Seville, 1949), pp. 452–5.
75 M.Moreyra y Paz Soldán, *En torno a dos valiosos documentos sobre Potosí. Los quintos reales y las pragmáticas secretas sobre la moneda* (Lima, 1953).
76 Sella, op. cit., pp. 117–18.
77 I shall return to this parabolic curve to discuss the relationships with other sectors of the Venetian economy.
78 Sella, op. cit., p. 125.
79 Ibid.
80 This trend in Venice finds confirmation in Milan: see C.Santoro, *Matricola dei mercanti di lana sottile di Milano* (Milan, 1940), p. 109; in Naples: see G.Coniglio, 'La rivoluzione dei prezzi nella città di Napoli nei secoli XVI e XVII', in *Atti della IX Riunione Scientifica della Società Italiana di Statistica* (Spoleto, 1952), pp. 236–7; and for Florence: see R.Romano, 'A Florence au XVIIe Siècle', p. 512. And for the situation in Italy in general—especially in the North—see G.Aleati and C.M.Cipolla, 'Il trend economico nello stato di Milano durante i secoli XVII e XVII: il caso di Pavia', *Bollettino della società pavese di storia patria,* I-II (1950); C.M.Cipolla, 'The decline of Italy: the case of a full matured economy', in *Economic History Review,* V (1952).
81 See note 73 above. Nevertheless, the qualitative composition of these exports changed considerably during the first half of the seventeenth century. Basically, the percentage of 'undressed' cloth became less in the total exports of London. See Supple, op. cit., p. 137.
82 For these aspects, I refer to the excellent book of A.Friis, *Alderman Cockayne's Project and the Cloth Trade. The Commercial Policy of England in its Major Aspects, 1603–1625* (London and Copenhagen, 1927), which will remain the definitive book on the study of commercial-industrial life of England during these years. Illuminating details may be gleaned from E.Moir, 'Benedict Weber, clothier', *Economic History Review,* X (1957).
83 See Hinton, op. cit., pp. 227–9.
84 T.C.Mendenhall, *The Shrewsbury Drapers and the Welsh Wool Trade in the XVI and XVII Centuries* (Oxford, 1953), pp. 234–5.
85 N.W.Posthumus, op. cit., II, p. 129; III, pp. 930–1. The total figures of production are as follows:

1591–1600	500,840
1601–10	669,431
1611–20	875,688
1621–30	1,001,293
1631–40	989,934
1641–50	892,835
1651–60	1,061,014

86 N.W.Posthumus, op. cit., III, p. 1163.
87 Ibid., p. 933.
88 Ibid., pp. 941–6.
89 Is it not significant that it was above all the dyers who developed after 1623? See J. C.van Dillen, *Bronnen tot de Geschiedenis van het Bedrijfsleven en het Gildewezen van Amsterdam,* II (The Hague, 1933), p.xix.
90 Van Dillen, op. cit., I (Gravenhage, 1929), p.xxi and II, p. 150, n. 270; p. 375, n. 642.
91 S.C.van Kampen, *De Rotterdamse particuliere Scheepsbouw in de tijd van de Republiek* (Assen, 1953), especially p. 109; see also pp. 33, 34, 106, 192. Also van Dillen, op. cit., II, p.xv.
92 Van Dillen, op. cit., p.xxx; van Kampen, op. cit., p. 148.
93 Van Dillen, op. cit., I, p. 585, n. 986; II, p.xxviii and p. 438, n. 757.
94 Ibid., I, pp. 592–6.
95 Van Dillen, op. cit., vol. 2, p.xxx.
96 Posthumus, op. cit., II, p. 70 and III, p. 890; van Dillen, op. cit., I, p.xxxii, II, p. xxxi.
97 For the development of urbanization in Amsterdam in the sixteenth and seventeenth centuries, see van Dillen, op. cit., I, p.xxv. To appreciate fully the influx into Amsterdam between 1615 and 1619, it is necessary to remember that in 1617 there was an epidemic in the city: see ibid. and p. 255. Also see Brugmans, op. cit., II, pp. 4ff.
98 Van Dillen, op. cit., II, pp. 16–17.
99 Ibid., p. 438, n. 757.
100 E.Coonaert, *La Draperie-Sayetterie d'Hondschoote* (Paris, 1930), p. 494.
101 For a detailed bibliography, see G.Aubin and A.Kunze, *Leinenerzeugung und Leinenabsatz im östlichen Mitteldeutschland zur Zeit der Zunftkämpfe* (Stuttgart, 1940), *passim* and especially pp. 290–361; E.Zimmermann, 'Der schlesische Garn- und Leinenhandel mit Holland im 16 und 17 Jahrhundert', *EconomischHistorisch Jaarboek,* XXVI (1956); E.Sabbe, *De Belgische Vlasnijverheid,* I (Brussels, 1953), pp. 331–45.
102 Chaunu, op. cit., VIII. ii, 2, pp. 1974–5.
103 Ibid., p. 1970.
104 Moreyra y Paz Soldán', op. cit., pp. 37–9.
105 B.H.Slicher van Bath, *De agrarische Geschiedenis van West-Europa (500–1850)* (Utrecht and Amsterdam, 1960), p. 226, and also 'Agriculture in the Low Countries (*c.* 1600–1800)', in *X Congresso Internazionale di Scienze Storiche,* IV (Florence, 1955), pp. 169–203; J.M.Kulischer, *Storia Economica del Medio Evo e dell' epoca Moderna,* II (Florence, 1955), pp. 61–92.
106 See Kulischer, op. cit., pp. 92–146.
107 E.Sereni, *Storia del Paesaggio Agrario Italiano* (Bari, 1961), p. 192; R.Romano, 'Rolnictwo i chlopi we Wloszech w XV i XVI wieku', *Prezgląd Historyczny,* LIII (1962), 2, p. 255.
108 F.Braudel, *La Mediterranée et le monde méditerranéen à l'époque de Philippe II* (Paris, 1949), pp. 616–42.
109 Romano, 'Rolnictwo', pp. 246ss.

110 L.Bulferetti, 'L'oro, la terra e la società. Un'interpretazione del nostro Seicento', *Archivio Storico Lombardo*, 8th series IV (1953), p. 44, n. 77.
111 A.Domeniconi, *La Compagnia dei molini di Cesena* (Faenza, 1956), pp. 182–3.
112 L.Zytkowicz, *Studia nad Gospodarstwem Wiejskim w Dobrach Koscielnich XVlw*, 2 vols (Warsaw, 1962).
113 Ibid., II, p. 51.
114 J.Topolski, *Gospodarstwo Wiejskie w Dobrach Arcybiskupstwa Gnieznienskiego od XVI do XVIII Wieku* (Poznan, 1958), pp. 216–17.
115 J.Majewski, *Gospodarstwo Folwarczne we Wsiach Miasta Poznania w Latach 1582–1644* (Poznan, 1957), pp. 80–8.
116 A.Wyczański, 'Le niveau de la récolte des céréales en Pologne du XVIe au XVIIIe siècle', in *Première Conference Internationale d'Histoire Economique (Stockholm, 1960). Contributions, Communications* (Paris and The Hague, 1960), pp. 585–90.
117 M.Malowist 'L'évolution industrielle en Pologne du XVIe au XVIIe siècle', in *Studi in onore di Armando Sapori*, I (Milan, 1957), pp. 589, 592; M.Malowist, 'The economic and social development of the Baltic countries from the fifteenth to the seventeenth centuries', *Economic History Review*, XII (1959). I am greatly indebted to my friend Professor Malowist for everything concerned with Poland in my article.
118 Slicher van Bath, op. cit., pp. 222–3.
119 Ibid., pp. 223–4.
120 M.Bloch, *Les Caractères originaux de l'histoire rurale française* (Oslo, 1931), *passim* and especially pp. 131–53.
121 J.Costa, *Colectivismo agrario en España* (Madrid, s.d.) J.Klein, *The Mesta. A Study in Spanish Economic History* (Cambridge, Mass., 1920).
122 Kulischer, op. cit., pp. 131–43.
123 M.Dobb, *Studies in the Development of Capitalism* (London, 1950), especially p. 194, n. 2.
124 I.J.Brugmans, 'De Oost-Indische Compagnie en de welvaart in de Republiek', *Tijdschrift voor Geschiedenis*, LXI (1948), p. 230; Klerk de Reus, op. cit., p. 118.
125 N.W.Posthumus, 'The tulip mania in Holland in the years 1636 and 1637', *Journal of Economic and Business History*, 1 (1928–9), pp. 434–66.
126 G.Alivia, 'Di un indice che misura l'impiego monetario dell'oro relativamente a quello dell'argento e le sue variazioni dal 1520 ad oggi', *Giornale degli Economisti* (1911), p. 346.
127 E.J.Hamilton, 'Monetary inflation in Castile (1598–1600)', *Economic History*, II, pp. 177–212: the 'premiums on silver in terms of vellón', which rose somewhat between 1604 and 1623 (from 3 to 8.90 per cent), increased in 1624 to 22 per cent, and maintained a steady level throughout the period examined by Hamilton; see also E.J.Hamilton, *American Treasure and the Price Revolution in Spain, 1501–1650* (Cambridge, Mass., 1934), particularly pp. 93–4.
128 For this, see W.A.Shaw, *The History of Currency* (London, 1895); A.Despaux, *Les Dévaluations monétaires dans l'histoire* (Paris, 1936). For Danzig, see J.Pelc, *Ceny w Gdansku w XVI i XVII Wieku* (Lvov, 1937), pp. 4–5; for Milan: A. de Maddalena, *Prezzi e aspetto di mercato in Milano durante il secolo XVII* (Milan, 1949), pp. 151–2; for Strasbourg: A.Hanauer, *Etudes économiques sur l'Alsace ancienne et moderne*, I (Paris and Strasbourg, 1876), p. 257; for Amsterdam: N.W Posthumus,

Nederlandsche Prijsgeschiedenis, I (Leiden, 1943), p.cxix. In any article about the history of prices, there are references to fluctuations in the value of money, confirming much of what I said on pp. 153–5 above. In addition, purely monetary fluctuations are found also in places that are very far from western Europe: in Moscow (Spooner, op. cit., p. 239); in Ragusa (V.Vinauer, 'Monetarna kriza u Turskoj', *Istoriski Glasnik,* III-IV [1958], p. 143); in Constantinople (as my friend Halil Sahilloglu, of the University of Istanbul, has informed me). For a general account of the problem, see the major work of R.Gaettens, *Die Zeit der Kipper und Wipper der Inflationen* (Munich, 1955), pp. 74–99.

129 F.C.Spooner, *L'économie mondiale et les frappes monétaires en France (I493–1680)* (Paris, 1956), pp. 524–9.

130 C.M.Cipolla, *Mouvements monétaires dans l'Etat de Milan (1580–1700)* (Paris, 1952), pp. 43–4.

131 Ibid., p. 35.

132 F.Mauro, *Le Portugal et l'Atlantique au XVIIe Siècle (1570–1670)* (Paris, 1960), pp. 415, 428 and 432.

133 Ibid., p. 417.

134 J.D.Gould, 'The Royal Mint in the early seventeenth century', *Economic History Review,* V (1952), pp. 240–8. A good general examination may be found in R. Ashton, *The Crown and the Money Market (1603–1640)* (Oxford, 1960). See also B.E.Supple, 'Currency and commerce in the early seventeenth century', *Economic History Review,* X (1957).

135 Hamilton, *American Treasure,* pp. 42–3.

136 W.A.Shaw, *History of Currency* (London, 1895), p. 144.

137 C.M.Cipolla, 'Note sulla storia del saggio d'interesse', *Economia Internazionale,* V (1952), p. 14.

138 Ibid., p. 16.

139 L.de Rosa, 'Il banco dei Poveri e la crisi del 1622', *Rassegna Economica,* XXII (1958); C.di Somma, Il *Banco dello Spirito Santo dalle Origini al 1664* (Naples, 1960). And also M.de Stefano, *Banchi e Vicende Monetarie nel Regno di Napoli (1600–1625)* (Livorno, 1640).

140 de Rosa, op. cit., p. 57.

141 di Somma, op. cit., p. 89.

142 de Rosa, op. cit., p. 65.

143 N.Piccolomini, *Il Monte dei Paschi di Siena e le aziende in esso riunite,* II (Siena, 1891), pp. 308–9.

144 Ibid., p. 215.

145 A.Hanauér, *Etudes économiques sur l'Alsace ancienne et moderne,* I (Paris and Strasbourg, 1876), pp. 560–6.

146 Ibid., pp. 586–7.

147 J.G.van Dillen, *Bronnen tot de Geschiedenis der Wisselbanken (Amsterdam, Middelburg, Delft, Rotterdam),* II (The Hague, 1925), p. 985. See also The Bank of Amsterdam' in J.G.van Dillen (ed.), *History of the Principal Public Banks,* (The Hague, 1934), pp. 177ff.

148 Van Dillen, *Bronnen,* II, p. 1304.

149 Ibid., p. 962.

150 Ibid., p. 991.

151 J.Gentil da Silva and R.Romano, 'L'histoire des changes: les foires de "Bisenzone" de 1600 a 1650', *Annales E.S.C.*, XVII (1962).

152 See G.Luzzatto, 'Les banques publiques de Venise (siècles XVI-XVIII)' in J.G. van Dillen (ed.), *History of the Principal Public Banks*, (The Hague, 1934), p. 49.

153 There is a vast bibliography on this subject. See particularly G.Imbert, *Des Mouvements de longue durée Kondratieff* (Aix-en-Provence, 1959), pp. 194–202. See also W.Beveridge, 'Weather and harvest cycles', *Economic Journal* (1921), p. 452, and also the excellent analysis of G.Parenti, *Prezzi e mercato del grano al Siena* (Florence, 1942), p. 235.

154 In addition to what has been said about the agrarian contraction after the end of the century, see Parenti, op. cit., p. 216.

155 F.Braudel, P.Jeannin, J.Meuvret and R.Romano, 'Le déclin de Venise au XVII siècle' in *Decadenza economica Veneziana nel secolo XVII* (Venice and Rome, 1961), p. 33, n. 1: in Lyons, in 1619, 6,000 silk-weavers were receiving 'distribution *de l'aumône générale*'.

156 A.de Maddelena, 'I bilanci dal 1600 al 1747 di una azienda fondiaria lombarda. Testimonianza d'una crisi economica', *Rivista Internazionale di Scienze Economiche e Commerciali*, II (1955). Confirmation may be found in another article by de Maddelena: 'Contributo alla storia dell'agricoltura della "bassa" Lombardia', *Archivo storico Lombardo* (1958), pp. 165–83. In addition to this it seems to me that, on another level, de Maddelena's article supports much of what I have said concerning the trend of agriculture on Italy, exemplified in the Company of Millers of Cesena.

157 de Maddelena, 'I bilanci', p. 44.

158 M.Morineau, *Incroyables gazettes et fabuleux métaux: les retours des trésors américains d'après les gazettes hollandaises* (Cambridge and Paris, 1985).

159 R.Romano, *Conjonctures opposées. La 'crise' du XVIIe siècle en Europe et en Amérique ibérique* (Geneva, 1992; Italian edition, Venice, 1992; Spanish edition, Mexico, 1993). See also the remarks of the editors of this volume on pp. 19–20.

160 F.Braudel, *Civilisation matérielle, économie et capitalisme, XVe—XVIIIe siècles* (Paris, 1993), pp. 793ff.

[*Editors' note:* many of the series of economic data discussed in this chapter are now available in G.Parker and C.H.Wilson (eds), *Introduction to the Sources of European Economic History, 1500–1800* (London, 1977).]

8
THE CRISIS OF THE SEVENTEENTH CENTURY IN SOUTHEAST ASIA*

Anthony Reid

The retreat of Southeast Asian trade in the seventeenth century has usually been explained in terms of the military and economic victories of the VOC,[1] to which is sometimes added the rise of interior, agrarian-based states less interested in commerce. These factors appear specific to the historical situation of Southeast Asia and particularly to the Indonesian Archipelago, where Dutch power was concentrated and the indigenous regimes were particularly vulnerable to monopoly pressure. Yet the parallels in other parts of the world cannot be ignored. After three decades of vigorous interaction with the world outside, Japan in the 1630s forbade its people to sail overseas and limited foreign trade to Chinese and Dutch ships at Nagasaki. China experienced a drastic period of famine, population decline and internal disintegration in the 1630s and 1640s, culminating in the collapse of the Ming regime in 1644 and its replacement by the Ch'ing. England, France, Germany, Spain, and Turkey were all convulsed by ruinous civil wars in the period 1620–50, and the last three of these regions at the same time lost their former prosperity and status in the world. The population of England, which had risen at between 0.5 and 1.0 per cent per annum throughout the expansive sixteenth century, slowed markedly from 1600 and went into dramatic decline from 1656 to 1686.[2] Though population movement is less well documented elsewhere, there is little doubt that it declined significantly in France, Denmark and Germany, as in China, in the second half of the century, while the setback occurred somewhat earlier in Italy and Spain. These demographic reverses in different parts of the world were accompanied by a decline in prices for grain and other essentials after a long period of price inflation.

This remarkable coincidence, in many parts of the world, of sixteenthcentury expansion followed by seventeenth-century crisis has led to a growing scholarly industry attempting to provide some general explanation. Earlier theories focused on political phenomena—for Roland Mousnier the conflict of nobility and bourgeoisie, and the undermining of supra-national ideas of both Empire and Church by the rising power of nationalism; for Hugh Trevor-Roper the top-heavy absolutism of the 'Renaissance state' with its enlarged army, bureaucracy and taxation demands.[3] For a number of Marxist-influenced scholars, of whom Eric Hobsbawm is the most explicit but Perry Anderson and Barrington Moore the

most wide-ranging, these were the birth-pangs of the modern world, when 'capitalism at last burst through the chrysalis of the old feudal order'.[4] Robert Mandrou and others have looked to cultural factors, seeing the baroque as an indication of the 'romantic agony' of European civilization in crisis. Most recently Jack Goldstone has argued that for three diverse countries—England, Turkey and China—the pressure of rapidly rising population on limited grain resources pushed prices up in the sixteenth century and correspondingly down in the seventeenth.[5]

Many of these points are suggestive in relation to the Southeast Asian evidence. In particular, the Trevor-Roper thesis that there was inherent instability in the greatly expanded resources taken by the absolutist state provides an interesting comparative insight on Siam, Aceh and Banten. However, only two of the theories advanced appear sufficiently general and sufficiently measurable to enable us to say with any confidence whether there is more than coincidence at work. The first of these focuses on the depression in global trade and financial indices in the period 1620–50, caused in part by the reduced supply of lubricating silver and gold. The other is the increasing body of data on global climatic change, which makes it clear that a major cooling of the earth's surface occurred in the seventeenth century.

TRADE DECLINE

Although debate continues about dating the precise turning-point, the evidence for a marked downturn in global trade in the first half of the seventeenth century is overwhelming. This is best documented by some of the vital indices of European trade: the shipping of Seville went into steady decline from 1622; Dutch shipping to the Baltic from 1620; the trade of Danzig from 1623; Venetian trade and cloth production from about 1610; production of Leiden textiles and Amsterdam soap from 1620; English wool exports from 1614; English real wages from about 1590, and so forth. European prices which had been on the rise for a century generally declined from around 1640.[6]

One specific cause of the trade downturn was the renewed outbreak of war between Spain and the Netherlands in 1621, which decimated Dutch trade to the Iberian peninsula and the Mediterranean and helped plunge Spain further into depression.[7] A more profound factor may have been an agricultural crisis, avoided only by Holland and England, which ensured that the Mediterranean and German economies did not quickly recover from the setback of the 1620s.[8] A critical phenomenon whose existence is no longer in doubt, though its effects are much debated, was the collapse after 1630 in the output of the Potosí silver mine (in what was then Spanish Peru). Even when the more stable output of the Mexican mines is added, there was a decline of 0.3 per cent a year in total American production of silver throughout the period 1628–97, contrasting with a spectacular growth before this period and a steady expansion after it.[9]

Outside Europe and its New World in the Americas, the effects of this economic downturn are less clear. Louis Dermigny has pronounced that 'in the general depression which characterized the world of the seventeenth century, the Chinese crisis was the most profound of all',[10] and there is little doubt that the drastic decline in imports of silver to fuel an overheated economy was one of the causes.[11] A case can be made that the effects were as profound in Southeast Asia, but the latter's much more favourable ecological situation meant that the effects showed less in drastic population declines than in a reduced commercial involvement.

Since both Spanish and Japanese silver flowed to China through Southeast Asia, the effect on Indonesia's ports was profound. Whereas an average of over twenty-three tonnes of specie had been shipped from Mexico to Manila each year in the 1620s, this fell to eighteen tonnes in the 1630s and ten tonnes in the following decade (see Table 8.1). Although some of this decline can be attributed to the reduced number of Chinese ships reaching Manila during the final crisis of the Ming dynasty, the shipments did not return to the peak levels of 1610–30 until well into the eighteenth century.

Portuguese trade and remittances of specie to Asia declined even more rapidly after 1600, and the Southeast Asian branch of this trade centred in Melaka was in deficit by 1630.[12] Even the VOC, the undoubted 'winner' of the seventeenth-century crisis in Asia, took less bullion to the east in the period 1630–60 than it had in the 1620s. In 1652, admittedly an exceptional year, the VOC in fact acquired more than 80 per cent of its silver and gold needs from Asia.[13] Until 1668 the greatest source of silver for the VOC, as for Asia in general, was Japan. The output of the mines on Sado Island ran curiously parallel to that of Potosí, rising spectacularly in the same period 1590–1630. Japanese exports of silver are believed to have reached a peak of 150 metric tonnes a year in the 1620s, only to decline to less than half that amount by the 1640s (see Table 8.1), while silver export was prohibited altogether in 1668. This remarkable boom and bust impacted directly on the Southeast Asian ports where Japanese silver was exchanged for Chinese silk and local deerskins and spices. The prohibition of Japanese trading abroad in 1635 and the resumption of direct (if tightly controlled) China-Japan exchange (through Chinese junks trading to Nagasaki) had a severe effect on the economy of all these Southeast Asian cities. The VOC's privileged access to Japanese minerals after 1639 through its factory at Deshima became a critical factor in the success of the Dutch world economy in Asia.

The Chinese trade to Southeast Asia, which had contributed much to the boom of 1570–1630, also slumped during China's mid-century troubles. Whereas 117 junks had been licensed to sail to the south in 1597, and probably more than that in the following decade, only thirty-nine passes were issued when the licensing system was briefly revived in 1639. Of these, twothirds were for the European enclave ports—sixteen for Manila and eight for Batavia; of the Asian ports only Cochin-China (eight) received more than two licences.[15] Yet 1639 was a good year by the standards of the midseventeenth century. In that year, thirty-four

arrivals from the Chinese mainland or Taiwan were recorded in Manila, which suggests that the officially sanctioned junk trade may have been about half the total.[16] After this the collapse was unremitting. An average of only seven vessels a year from Chinese ports reached Manila in the period 1644–81, and less than five reached Batavia, which had become the principal Chinese destination in the southern islands.[17]

Table 8.1 Supplies of silver and gold in eastern Asia (decennial annual averages in metric tonnes of silver equivalent)[14]

	Portuguese	*VOC*	*English*	*Manila galleon*	*Japanese exports*
1581–90	8.6			4.0	30
1591–1600	?			2.7	40
1601–10	5.9	5.7	1.3	12.0	80
1611–20	4.7	10.9	4.7	19.4	110
1621–30	4.4	12.7	7.7	23.1	130
1631–40		8.7	5.5	18.4	130
1641–50		9.5	?	10.1	70
1651–60		8.6	?	9.0	50
1661–70		11.8	9.9	8.0	40

Although most sections of the world economy faltered in this period, Southeast Asia was especially hard hit as its share in the shrinking pie was reduced. The region had benefited more than most others from the trade boom of 1570–1630. International competition for its products reached a peak in the 1620s, when pepper and Malukan spices accounted for more than half of the value of Dutch, English, Portuguese and French homeward cargoes, while most of the trade between China and Japan also took place in Southeast Asian ports. However, in 1635 the Japanese stopped coming altogether, while the crisis of the 1640s drastically reduced Chinese demand. For Europeans, other Asian interests, notably Indian cloths and indigo, replaced the initial concentration on Indonesian spices and pepper. Malukan spice and Southeast Asian pepper together dropped from 68 per cent of Dutch homeward cargoes in 1648–50 to 23 per cent in 1698–1700, and from the bulk of English cargoes before 1640 to about 2 per cent after the loss of access to Banten in 1682.[18]

Of all Southeast Asian ports only Dutch Batavia grew in stature in the latter half of the century. Although Spanish Manila suffered fewer pressures than most Asian entrepôts, its trade also declined markedly, as the series of Chaunu (Table 8.2) amply demonstrates.[19]

Table 8.2 Imports of Manila (five-year averages in Spanish reales)

Period	*Cargo entered*	*Customs charged*
1601–05	179,168	43,037
1606–10	239,832	42,982
1611–15	474,866	70,356
1616–20	615,599	51,437
1621–25	no figures available	
1626–30	492,866	25,720
1631–35	567,135	42,194
1636–40	577,813	31,037
1641–45	566,208	22,075
1646–50	379,535	14,316
1651–55	192,094	7,504
1656–60	214,904	6,676
1661–65	277,736	4,858
1666–70	186,177	3,884

The downturn in Manila, as in the Asian-ruled ports, was not made good by an improvement in Chinese trade after its nadir in the 1640s and especially after the defeat of the Cheng (Koxinga) rebels and the opening of trade in 1684. As will be discussed below, the Chinese junk traders fared better than any other Asians in the second half of the seventeenth century. Nevertheless, the only branch of this commerce for which reliable figures survive, the junk trade which the Japanese permitted under tight conditions to Nagasaki, declined from Cambodia and Patani after 1660, from Cochin-China after 1680 and from Ayutthaya after 1689 (Table 8.3).[20]

Table 8.3 Junk trade from Southeast Asia to Nagasaki

Decade	*Tongking*	*Cochin-China*	*Cambodia*	*Siam*	*Patani*	*Banten*	*Asian total*	*Dutch ports*
1651–60	15	40	37	28	20	1	141	2
1661–70	6	43	24	26	9	—	108	14
1671–80	8	41	10	26	2	1	88	38
1681–90	12	25	9	31	9	—	86	23
1691–1700	6	29	23	19	7	1	85	18
1701–10	3	12	1	11	2	—	29	2

The effective monopoly established by the VOC over nutmeg and mace in 1621, and progressively over cloves between 1640 and 1653, dealt the most serious single blows to Southeast Asian trade, even though the quantities involved were much less than for pepper. These items had been the staples of the 'long-distance trade' for centuries, passing though dozens of hands as they travelled from producers in Maluku [the Moluccas] to markets in Java, Makassar or the Malacca Straits, and thence progressively to India, the Middle East, Europe or China. Both prices and quantities for these spices had been at their maximum in the first quarter of the seventeenth century, as the northern Europeans competed for supplies with each other and the older-established Chinese, Portuguese and Muslim buyers. Until this period, the fact that clove and nutmeg grew only in eastern Indonesia served to draw wealth eastward, invigorating a dozen different ports along the trade routes. Once the Dutch monopoly was established, however, prices to the growers were fixed at minimum levels, and all Asian intermediary traders and ports were eliminated from a share in profits. By the mid-seventeenth century the Dutch sold spices in Europe at about seventeen times, and in India at about fourteen times, the price for which they had bought them in Maluku, and none of the profits passed into Asian hands.[21]

The mid-seventeenth century can thus undoubtedly be considered a period of crisis for the trade of Southeast Asia. In difficult times of lower prices and reduced demand from both China and Europe, there was room for only one winner. The VOC was that winner, and its profits reached peak levels in precisely the period of crisis in other parts of the global economy The prices of VOC shares on the Amsterdam stock exchange were high throughout the period 1640–71, with record prices in 1648 and 1671.[22] But these profits were only possible, given the very high military and administrative overheads, because the VOC used its power selectively to establish monopoly conditions. All the other actors in the Southeast Asia trade lost out in this period— Spanish, Portuguese, Gujaratis, Bengalis, Chinese, Japanese and English, and above all the Southeast Asian trading groups. Eventually the VOC itself suffered a period of stagnation, as its homeward cargoes from Batavia dipped in the period 1660–1700, and this can be attributed at least in part to the impoverishment of the Southeast Asian trade on which it had previously relied.[23]

CLIMATE

The most truly global explanation of the 'general crisis' is also the least understood: the gradual decline in temperatures during the seventeenth century (perhaps caused by the 'Maunder minimum' of solar activity: see Chapter 11 in this volume), reaching its nadir in many parts of the northern hemisphere in about 1690 before the modern warming trend began. Evidence has been accumulating recently to substantiate both the global nature of the cooling process and the

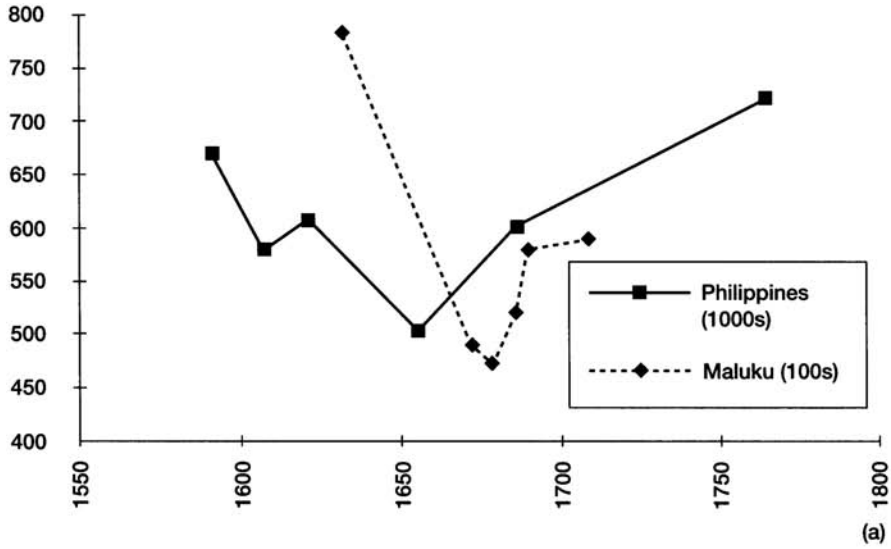
negative effects of such cooling on harvests and grain yields in the northern temperate zone.[24] Equally important is the evidence that periods of global cooling coincide precisely with the greatest variability in climatic conditions.[25] Parallel research has been conducted on the effects of short-range fluctuations, which have shown that a correlation exists between El Niño events and droughts in Java.[26] Although too little is yet known about the interaction of short-term and long-term cycles in tropical areas such as Southeast Asia, there can no longer be any doubt that global climatic factors do influence both disease mortality and famines induced by drought.

The effects of the Little Ice Age on the humid tropics remain unclear, though they probably included a greater variability of short-term changes in the weather. There are few systematic runs of climatic data in Southeast Asia which can help establish whether the seventeenth century was also climatically difficult for Southeast Asia. The best is Berlage's remarkable series of tree-rings from the teak forests of Rembang in east-central Java, which provides relative rainfall levels for every year between 1514 and 1929 (see Figure 8.1).[27] This shows 1598–1679 to have been the worst substantial period for rainfall in the 415 years recorded: of the 82 years in that dry period, only 13 reached the mean level of rainfall over the four centuries. Not a single year between 1643 and 1671 did so, marking these out as much the most critical decades.

Distinguishing the long-term from the short-term fluctuations, and drawing conclusions for neighbouring areas from the data on Rembang, remain fraught with difficulty. Other sources do suggest a correlation between the Berlage data on dry periods in Java and crisis events in a broader area of the Archipelago. We might indeed expect that in those areas of the eastern Archipelago where survival depended on a delicate balance between wet and dry monsoons, these years of low rainfall would lengthen the dry season dangerously, giving rise to crop failures, famines, dry and polluted wells and epidemics.

The first critically dry period in Berlage's series was 1605–16. Each of these years was markedly below average and 1606 was the second driest year (after 1603) recorded since 1580. Although descriptive sources are not abundant in this period, both Dutch and Malay records note the period 1606–8 as one of exceptional famine in Aceh (partly caused by war), from which many people died. This bad period may also help explain the epidemic said to have reduced Kedah's population by two-thirds in 1614, and the 'great reduction in population' in northern Maluku which VOC Governor-General Laurens Reael attributed mostly to warfare.[28]

Although a bad rice crop afflicted Makassar in 1624 and terrible epidemics hit Java in 1624–5, the 1620s appear relatively benign according to the Java tree-ring data. The next bad patch in Berlage's series was a shorter one from 1633 to 1638, with 1634 and 1637 the worst years. This may be connected to the rice shortage reported for Bali in 1633, the drought in Maluku in 1635, and the terrible epidemic in Makassar in 1636 in which 60,000 people were said to have died in



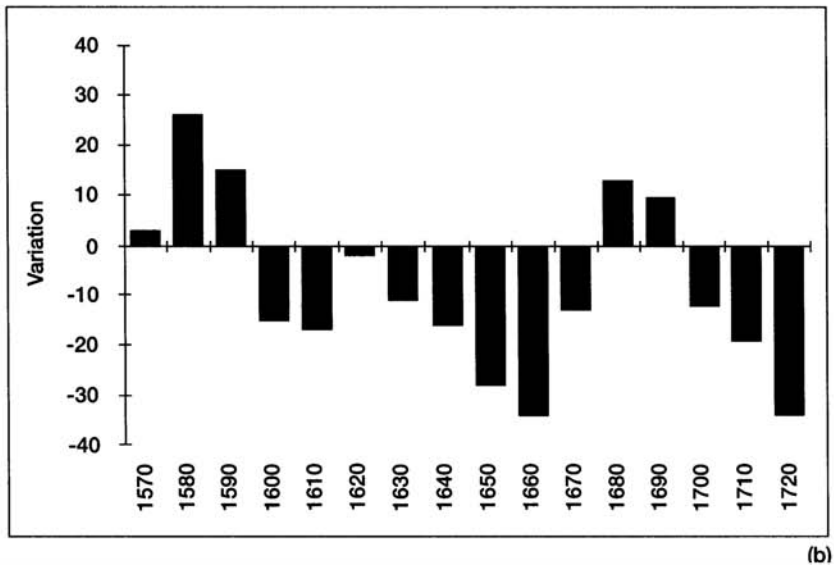


Figure 8.1 The seventeenth-century crisis as reflected in population and climatic indicators

Note: (a) Population of Philippines (thousands) and Dutch-controlled Maluku (hundreds); (b) tree-ring growth in Java, in terms of variation by decade from a fourhundred-year mean, in decades beginning in the years indicated.

forty days. Although there was also a prolonged drought and crop failure in Siam in 1633, this should probably be seen as part of a different weather cycle.[29]

As mentioned above, the most dramatic feature of the whole Berlage series is the severe dry period revealed from 1643 to 1671. The importance of these data is confirmed by the drought in Banten in 1657 and 'the great drought' of 1660–1 recorded in southern Borneo, in Ambon and in 'most quarters of the Indian Archipelago', as well as in Palembang, where the Musi river was so low that Dutch vessels could not pass. The grim nadir of this period came in 1664 and 1665, the driest of any in the whole 400-year series of Java treerings. The second and worst of these years witnessed probably the most disastrous epidemic of the century, affecting most of Indonesia. The population of both the Makassar area and Java was said to have been 'very much lessened' by it, and Bali and the west coast of Sumatra also suffered.[30] Finally, the tree-ring data reveal another shorter bout of very dry seasons in 1673–5, which also shows up well in Dutch observations. Mataram suffered famine and illness throughout the years 1674–76. The rice crops of Jambi, Sumbawa and South Sulawesi failed in 1674, and in South Sulawesi (again) and Palembang in 1675, while the Musi River again dropped too low for navigation. Mataram suffered famine and illness throughout the years 1674–76, and Maluku in 1675.[31] Although Dutch factors tended to record these crisis events only when their own people or allies died, or when the high price of rice in a particular area affected their commercial calculations, there is enough confirmatory evidence to show that the exceptionally dry weather recorded in the tree-rings did seriously increase mortality.

The continental masses of Southeast Asia cannot be expected to follow the rainfall patterns of Java in any simple fashion. However, global cooling does appear to have created conditions of unusual variability in other parts of Asia. In the whole period 1626–40, China experienced exceptional difficulties, 'with extreme droughts being followed by major floods'. Some estimates show Chinese population dropping 40 per cent in consequence between 1585 and 1645.[32] Detailed local records exist for coastal southern China (Kwantung and Fukien) which interestingly do show severe drought in the worst years of the Java tree-rings—1664 and 1665. The most sustained dry periods in south China, however, occurred in the 1640s and again in the 1680s, which understandably correlate better with Vietnamese crisis events than those elsewhere in Southeast Asia.[33] The most severe drought-induced famine in Tongking appears to have come in 1641, whereas the Nguyen chronicles suggest that the period 1641–1700 was particularly severe in Cochin-China, with twenty crisis years recorded.[34]

Burma and western Siam appear to have had a long-term climatic pattern more similar to that of southeast India than to the pattern of the rest of the region. In India as a whole the worst years were 1630 and 1631, representing 'almost certainly the most destructive Indian famine of the early modern era', which killed about 3,000,000 people in Gujerat alone.[35] This spread to the east coast of India and beyond into Southeast Asia, where many people died in Burma and Arakan in 1631–2, and a prolonged drought destroyed most of the 1633 rice crop

in Siam. In 1660–61 another appalling drought struck South India, and particularly Tanjore, where 'famine has so increased that whole villages, towns and hamlets have been depopulated, and hardly anyone remains, the dead lying in dried-up tanks'. This too had some effect in Burma, where 1661 saw very high rice prices.[36]

Nevertheless, by the standards of seventeenth-century India, China, and Europe, weather-induced famines appear to have been relatively moderate in Southeast Asia. Europeans considered the region blessed by its climate and its abundance of alternative food supplies even if rice crops failed. The above indications of exceptional crisis mortality, particularly in 1640–75, would therefore not seem a plausible factor to explain broader political and economic setbacks, except for strong evidence of population decline in the same period. When each case of population loss is viewed separately, warfare usually appears the likeliest cause. Yet the overall demographic record shows such a conjuncture between different Southeast Asian areas and the broader global pattern that it becomes necessary to look for economic and climatic explanations too.

The demographic data for the seventeenth century are sparse, and the best derive from the areas of greatest European control—which for that very reason may not be typical. The Spaniards kept lists of the numbers of Filipino *tributos* from whom they claimed labour and tax in Luzón and the Visayas. Table 8.4 gives the totals (each *tributo* perhaps corresponding to 4 or 5 people).[37]

Table 8.4 Tributos in the Philippines

Year	*Number*
1586	146,700
1591	166,903
1608	125,196
1621	130,938
1655	108,277
1686	121,000
1742	184,814

Some of this loss may be explained by Filipinos fleeing Spanish exactions into the uncontrolled hills, but the 35 per cent loss between 1591 and 1655 is too great to be accounted for by this alone. It contrasts markedly with the rapid subsequent growth in Philippine population, which on these figures would be 0.35 per cent per annum after 1655 and 0.76 after 1686. The Spanish conquest, followed by the heavy taxation needed to sustain the war with the Dutch (1609–48), and the collapse of the China trade in the middle of the century, may have played a part in this striking mid-seventeenth-century population dip. But the chronological fit with the climatic data is difficult to ignore.

Population figures are also available for Ambon and the Lease Islands (in Central Maluku), relatively meticulously supervised by the VOC which concentrated all its clove cultivation there (see Table 8.5).[38] Gerrit Knaap

Table 8.5 Central Maluku population

	Ambon	*Lease*	*Total*
1634	22,670	18,565	41,235
1671	19,338	15,973	35,311
1674 (lowest)	17,609	16,596	34,205
1680	18,486	17,288	35,774
1685	19,262	18,847	38,109
1690	21,075	21,142	42,217
1695	22,167	20,940	43,107
1708	21,140	21,343	42,483

attributes this mid-century drop to the bitter wars fought by the Dutch for control of the clove-growing islands (1641–56), and to a major epidemic— perhaps malaria—in 1656–8. While these must be factors, they do not explain why population continued to decline long after the *pax neerlandica* was established in 1656, reaching its nadir in Ambon in 1674, in the 'western' Ambonese islands in 1673, and in the Lease islands in 1672. A rapid population expansion of about 1 per cent a year then restored the population of the Ambon area during the next two decades.[39]

These two data series are the only ones that are reasonably reliable at regular intervals through the seventeenth century, and the coincidence between them must give rise to speculation about both economic and climatic factors (see Figure 8.1). The larger states also exhibited some striking indications of population decline, though the figures are not precise or systematic enough to say with confidence when it occurred. The core area of the Mataram kingdom (most of modern central and east Java, without the *pasisir*) was reckoned by a Mataram tax registration in 1631 to be 500,000 *cacah* (taxable households), or perhaps 3, 000,000 people. At the division of the realm in 1755, there appeared to be only 172,500 *cacah*. The first of these figures is obviously conventionalized at best and not to be taken very seriously. However M.C.Ricklefs has convincingly shown that there was a consistent downward trend whenever conventionalized local figures were revised between 1651 and 1755, beginning with a major decline reported in 1678. After the Gianti peace of 1755 there was the same population boom as had begun earlier in the Philippines (and temporarily in Maluku).

In the relatively crowded Red River delta, the Vietnamese heartland known to foreigners as Tongking, tax records show some remarkable rises and falls in the number of villages recorded for revenue purposes (see Table 8.6).[40] This

Table 8.6 Villages taxed in Tongking

Year	*Number*
1417	3,385
1490	7,950
1539	10,228
1634–43	8,671
1760s	8,760
1810	10,635

is not an adequate indicator, since we cannot be sure that administrative villages remained of comparable size throughout the period. Nevertheless the rapid growth (0.5 per cent per annum) during the prosperous and peaceful fifteenth century is striking, as is the remarkable decline after 1539. Other evidence makes it probable that most of the decline occurred during the period of ferocious civil war between the Trinh and the Mac (1545–92), which in turn was connected with the worst series of disaster years recorded in the Vietnamese Annals—1559, 1561, 1570, 1571, 1572, 1586, 1588, 1589, 1592, 1594, 1595, 1596, 1597. Crop failure, starvation and disease in each of these years killed large proportions of the population in certain areas, and drove others to migrate to the southern frontier or elsewhere.[41] While one would have expected the population to rise again after this appalling period, its failure to do so in the seventeenth century (if the record of villages corresponds to population) suggests that rural conditions continued to be very much worse than they had been before 1539.

A combination of factors, including a much less favourable global commercial climate, Dutch monopoly pressure, military defeats, and a relatively unstable climate with a high incidence of drought, thus combined to produce an unusually severe crisis for Southeast Asia in the middle of the seventeenth century. The reduced opportunities for trade became ever more apparent between 1630 and 1650, while the climatic situation probably reached its nadir in the 1650s and 1660s. The effects of the crisis are observable in the demographic record, such as it is, but their most important long-term effect may have been to shift many Southeast Asian societies towards greater selfreliance and distrust of the international market.

RETREAT FROM THE WORLD ECONOMY

Commerce does not yield much profit,
even if you grow pepper, my friends.
If there is no rice in the country…
what good are purple headcloths or daggers with golden handles?
If there is no rice in the country,
rulers and princes will lose their stature…

> Even if you have quantities of gold, what use is it if you are starving?[42]

Ironically it was those with the most precious resources who were first driven to the conclusion that they should have nothing more to do with cultivating them. Just as Filipinos stopped exploiting their gold when they saw that it brought the unwonted attention of predatory Spaniards,[43] the growers of spices discovered in the seventeenth century that they were possessors of a very mixed blessing. The Banda Islanders suffered worst, virtually wiped out as a people in 1620–23 by Dutch determination to control their precious nutmeg trees. The people of Ternate, Tidore, Bacan and Makian in northern Maluku, who had monopolized the world's supply of cloves until the sixteenth century, turned against spice production in the early seventeenth. The ferocity of the war between Spanish-backed Ternate and Dutch-backed Tidore in the first two decades of that century was the primary reason the Malukans abandoned clove cultivation, in the hope they could escape destruction.[44] In addition to buying cloves at below market price, the VOC caused a fivefold rise in the price of rice after it took over supplying goods to the islands from the highly competitive Javanese and Malays. The result was a wholesale shift out of cash-cropping in northern Maluku:

> With our own eyes we see them everywhere plant whole fields with rice, beside their gardens, which they preferred not to do in the time when they had commerce with foreigners, for these people…paid with cloves for rice and sago, the bread of these islands, at a civil price and easily obtained; whereas now to avoid the high prices they either plant, as with rice, or go and look for it in other places, as happens with sago, which they get from Sula, Taliabu, as well as the islands of Bacan.[45]

Although the decline in clove production in northern Maluku was made good by the Ambon area, it too soon suffered from warfare and the drive for monopoly. Population fell, as we have seen, and so did the clove harvest.[46]

Others drew the obvious lesson. When a Dutch factor visited Magindanao in 1686, he was told:

> Nutmeg and cloves can be grown here, just as in Maluku. They were not there now because the old Raja had all of them ruined before his death. He was afraid that the Dutch Company would come to fight with them about it.[47]

Pepper-growing, too, lost its attraction when it became clear that the high prices of the period 1616–50 were over. Between 1650 and 1653 the price paid for pepper in Southeast Asian markets dropped by half. In the 1670s supply outran demand even more severely: the pepper price in Holland dropped temporarily by

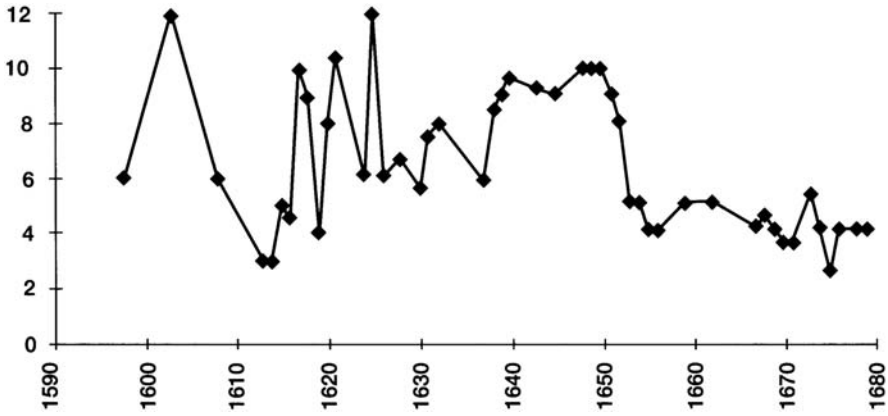


Figure 8.2 Prices paid for pepper in Southeast Asia, in reals per pikul

four-fifths, and prices paid in Southeast Asia were only a quarter of what they had been in the 1640s (see Figure 8.2).[48]

Some rulers decided to ban pepper cultivation because they felt the need for greater self-sufficiency in dangerous times. Early in the century (probably during the 1606–8 famines) the Sultan of Aceh had already ordered the destruction of pepper vines in the vicinity of the capital because his subjects 'amused themselves with nothing else, and neglected the cultivation of the ground, so that every year there was great scarcity of victuals'.[49] The English believed that Banten cut own its pepper vines around 1620 in the hope that this would encourage the Dutch and English to leave the Sultanate in peace, though self-sufficiency must have been an additional reason.[50]

Other states were forced to shift from cash crops to staple foods by Dutch blockades of their shipping, the favourite weapon of the VOC against cities dependent on food imports. Banten was the most frequently affected, and the 1630s saw a widespread shift to rice-growing. Not all could change their livelihood, however, and the English complained that, 'The long desistance from planting of pepper here, which commodity was the only trade of this place, hath so empoverished this people that many of them hath no other manner of living, but stealing.'[51] The Sultan of Magindanao told the Dutch in 1699 that 'he had forbidden the continued planting [of pepper] so that he did not thereby get involved in war, whether with the [Dutch] Company or with other potentates'.[52] Other rulers squeezed the dwindling profits of pepper-growing so hard that they killed the supply altogether. The Sultan of Palembang's desperate efforts to monopolize supplies of pepper to stop prices falling in the 1670s gravely reduced the supply from growers.[53] The similar policies of Jambi and Banten have already been noted.

The disenchantment with pepper-growing by the end of the century emerged very clearly in the Malay literature, which reflected the anxiety of court élites threatened by its instabilities. In Banjarmasin the court chronicle pleaded:

> Let people nowhere in this country plant pepper, as is done in Jambi and Palembang. Perhaps those countries grow pepper for the sake of money, in order to become wealthy. There is no doubt that in the end they will go to ruin. There will be much intrigue and food will become expensive… Regulations will be in disorder because the people of the capital will not be respected by those of the countryside; officers of the king will not be feared by the highlanders…
>
> If more [pepper than was needed for household uses] should be planted, for the purpose of making money, this would bring misery over the country…instructions from above would not be executed because the people would be emboldened against the king.[54]

Similar sentiments were expressed in eighteenth-century Aceh, as quoted at the beginning of this section. Not only did the court élite seek a return to greater self-reliance in view of the dangers and instabilities of the market. They also sought a more stable social order, in which *nouveau-riche* cash croppers and traders did not pose a threat to their status.

Pepper was by far the most valuable Southeast Asian export in the early modern period. When prices were still high, in the 1640s, this crop alone probably earned the equivalent of about 25 tonnes of silver, most of which was spent on imports. Production continued to rise despite lower prices until about 1670, when more than 8,000 tonnes were exported each year. Thereafter the quantity as well as the price dropped sharply and Southeast Asian growers began to return to subsistence crops on a much larger scale. The loss of Malukan spice profits was felt earlier, and the China-Japan trade was in crisis in the 1640s. The effect of this reduction in export revenue was apparent in the declining ability to buy imported Indian cloth.

Southeast Asian imports of cloth peaked in the period 1620–50, at around a million and a half pieces per year and a value (in Coromandel prices) equivalent to about 40 tonnes of silver. In the second half of the century textile prices rose markedly in India, just as pepper prices dropped in Southeast Asia. On the Coromandel Coast, the main source, purchase prices rose 45 per cent between 1665 and 1700. Because of the VOC near-monopoly in many Archipelago ports the rise in prices was greater there. Not surprisingly, the VOC was selling 20 per cent less in the Archipelago in 1703 than in 1652. VOC sales to vessels from Archipelago ports visiting Batavia to buy Indian cloth dropped more precipitately: by 43 per cent between 1665–9 and 1679–81, and even more rapidly in the subsequent twenty years.[55] Since Gujarati, Bengali, English and Portuguese supplies all dropped drastically in the middle of the century, there is

no doubt that overall Southeast Asian consumption of Indian cloth was in sharp decline

Dutch and British factors complained with increasing frequency that the peoples of the Archipelago were so impoverished by the loss of their export trade that they could no longer afford Indian cloth. After two decades of Dutch blockades and interruptions of its pepper trade, the population of Banten, 'formerly so opulent and prodigal in its daily clothing, now made a very impoverished and desolate [impression]' in 1634, and its people had been obliged to weave their own cloth.[56] In Central and East Java in 1684:

> The Javanese, having become poor and indigent as a result of the endless wars and upheavals, have been forced to resort, more than they otherwise would, to weaving their cloths themselves, not only for their own use, but also to sell these in other places.[57]

In the 1670s and 1680s, after the VOC conquest of Makassar and the subsequent loss of its spice trade, the Bugis and Makassar people were reported to have resumed their former weaving because they could no longer afford to buy Indian cloth.[58] Both Javanese and Makassarese took to selling their cheaper cloths around the Archipelago, increasing the popularity of Selayar check-cloth and Javanese batik respectively, and giving the impoverished populations elsewhere cheaper alternatives to turn to. The population of Jambi and Palembang began buying Javanese cloth in the 1690s, as well as weaving its own, because it could no longer afford the Indian cloth of which the Dutch had monopolized the supply by 1683.[59]

The *Hikayat Banjar* expressed this shift in ideological terms, as part of its rejection of the market economy: 'whenever people follow the style of dress of other countries, evil will certainly befall that country'.[60] A Dutch Governor-General, however, expressed the problem differently:

> The weaving of their own clothes has been in practice for the convenience of the ordinary people since olden times among the Javanese and most eastern people; but since these countries flourished formerly more than now, most of these peoples sought Coromandel and Surat cloths [for everyday use], not as luxuries, and gave large amounts of money for that…. Now most of the surrounding countries are impoverished, and the [Coromandel] Coast and Surat cloths have become limited to the use of the wealthy.[61]

A few years later another Dutch official pointed to the impossibility of preventing the Javanese from growing their own cotton and weaving their own cloth rather than buying from the Dutch:

> It is nothing but poverty that is the true reason that the traffic in the finest [Coromandel] Coast and Surat cloth declines daily while by contrast their own weaving has increased more and more through the multiplication of poor people.[62]

As the trade which had given life to the great maritime cities ebbed away from them, the cities themselves lost population and centrality. All of the major cities except the Vietnamese capital were conquered and pillaged at some point in the sixteenth and seventeenth centuries. Thang-long was a special case as capital of the most centralized Southeast Asian state and the one least interested in international trade. Yet even it appears to have declined from its fifteenth century peak—for reasons usually described in purely polit ical terms.[63] Of the other important cases only Ayutthaya, devastated by the Burmese in 1569, regained its previous population. The disasters that befell the remaining cities can be seen as crises in an underlying trend against maritime trade-based cities, so that the urban populations of the age of commerce were not recovered.

Melaka was reduced to about a quarter of its population as a result of the Portuguese conquest in 1511, and has only regained its former dimensions in our own times. Brunei was substantially smaller after its destruction by the Spaniards in 1579, even though the conquerors did not stay. Pegu was never again a major city after its terrible ordeal of 1598–9, and observers in subsequent centuries could only marvel at the dimensions of its ruined walls. Surabaya, thought to have had a population of 50,000 before its destruction by Mataram in 1621–5, did not reach this level again until the nineteenth century; Makassar and Banten, conquered by the Dutch in 1666–9 and 1682 respectively, lost their economic and political *raison d'être,* and declined to no more than a quarter of their previous populations.

The economic function of these entrepôts was assumed by the European enclave cities which took over the most lucrative long-distance trade of the region. Because they played a minimal role as political and cultural centres for their hinterlands, and because they discouraged the influx of Asian dependants which the older cities had attracted or forced to them, they remained much smaller than their predecessors. Portuguese Melaka never rose above 30,000, and even Batavia had scarcely 30,000 inhabitants within its walls in the second half of the seventeenth century when it dominated the maritime trade of Asia. The commercial life and urban values which flourished behind the walls of these colonial cities had minimal influence on the majority populations of Southeast Asia.

The urban-centred world of Southeast Asia had vanished by 1700. When census data became available in the late nineteenth century the region was one of the world's least urbanized.

RESPONSES TO THE LOSS OF TRADE REVENUE

The Mainland states and Aceh proved strong enough militarily, or uninteresting enough commercially, to avoid the fate of Makassar and Banten: all retained their independence and their freedom to trade with whomever they wished. They nevertheless went through their own crises in the seventeenth century, which saw them draw back from their dependence on foreign, especially European, commerce. After the destruction of Pegu in 1598–9 and the subsequent adventure of Felipe de Brito in Syriam, the restored Toungoo kings of Burma shifted their capital definitively to Ava, in the rice-growing heartland of Upper Burma, in 1635. Ayutthaya in its '1688 revolution' expelled French traders and imprisoned French missionaries, limited contacts with other Europeans, and surrendered its place as one of the leading trade centres of Asia. The two Vietnamese states put increasing obstacles in the way of European trade at the end of the century. Laos, like Aceh, fell into disunity after its brief 'golden age' in the early seventeenth century. Champa's autonomy ended and Cambodia's was severely compromised by repeated military intervention from its neighbours.

Earlier European historians tended to see these developments negatively, as self-defeating attempts at isolation from the modern world. According to D.G.E.Hall 'no [Burmese] king after Anaukhpetlun [1606–29] appreciated the value of overseas intercourse… So the dynasty surrendered to traditionalism and isolationism.'[64] E.W.Hutchinson had no better view of the 'spirit of blind and arrogant self-sufficiency' in Siam after 1688.[65] For a number of reasons these developments have been re-evaluated in a more positive light by recent scholars.[66]

First, a broader consideration of the way states responded to the seventeenth-century crisis can leave little doubt that Japan was the most successful survivor in Asia, just as Holland was in Europe.[67] Curiously 1635, the year of Burma's 'retreat' to a stronghold in Ava, was also the year when the Japanese were forbidden to go abroad, one of a series of moves put in place by the Tokugawa shoguns in the 1630s to restrict contacts with the outside world. Recent historical work has demonstrated both that trade and cultural contact did nevertheless continue, and that the conditions of internal peace and a unified market created by the Tokugawa were favourable for capital accumulation and productivity increases.[68] Some degree of withdrawal from foreign trade became politically desirable, in other words, once that trade became associated with the military-backed demands of Europeans—for religious freedom or privilege on the part of the Iberians and the French; for monopoly on the part of the Dutch and English companies.

Second, the concentration by Western writers on relations with Europeans tells only half the story. While the major European actors were indeed expelled from the Mainland states or gave up trying to surmount the obstacles put in their path, Chinese private trade revived everywhere towards the end of the century. The Chinese factor was most important in the countries around the South China Sea,

especially in the two Vietnams where silks were the preferred dress and there had never been much interest in the Indian cottons brought in from the west. How damaging was the seventeenthcentury crisis for the economy of such states?

In Vietnam the major impact of the boom period had been the JapaneseChinese trade taking place at Hoi An in Cochin-China in 1600–35. The ending of this by the Tokugawa in the 1630s must have been a major setback. The Dutch were admitted in both Vietnams because they replaced the Japanese, bringing in Japanese silver, copper and copper coinage and taking silk back to Nagasaki. The VOC maintained a factory in Cochin-China in 1633–41 and 1651–4, and a longer-standing one in Tongking in 1637–1700. The virtually constant warfare between the two states in the period 1627–79 created another source of interest in foreign imports, with the Portuguese providing guns for the Nguyen and the Dutch for the Trinh. The Dutch complained bitterly about the difficult conditions for trade in both states, as did the English during their brief experiment with Tongking in 1672–97. The Vietnamese must have found the western barbarians troublesome and demanding, and preferred to deal with Chinese traders for their essential needs. The junk trade remained strong, and perhaps even increased after 1684, but the collapse of other branches of trade removed the largest sources of state revenue and left the Vietnamese states more dependent on a single source of goods and ideas.

The first half of the eighteenth century is portrayed by Vietnamese historians, largely following the chronicles, as a series of disasters, famines and peasant rebellions leading to the collapse of both Trinh and Nguyen regimes before the storm of the great Tay-son rebellion.[69] The fundamental reasons for this remain obscure. In the north the Trinh regime appeared to lose the capacity to mobilize the population effectively to maintain the dykes. In the south the decline of trade revenue induced the Nguyen to increase impositions on land and on upland tributaries, which they could not in the long run sustain. Even though the two Vietnams exhausted each other in warfare for much of the seventeenth century, war may have provided them with a discipline and a stimulus to innovate which peace did not.

In Burma there is no question that the seventeenth century was a bad time for maritime commerce. It was not only that the English withdrew their factory in 1657 and the VOC in 1679 after one decade and four decades respectively of struggling against restrictive practices and unprofitable trade. The destruction of Mon overseas trade proved permanent, and the flight of thousands more Mons into Siam during further bouts of rebellion and suppression in the 1660s and 1750s decimated Burma's merchant class. Lieberman believes that some recovery occurred in overseas trade with Burma's delta ports in the 1660s, but that it did not regain its sixteenthcentury levels until after 1800.[70]

Lieberman nevertheless refutes the concept of 'a long period of stagnation' in Burma starting in the mid-seventeenth century. While the maritime regions of the delta remained in disarray, the population of the core region of Upper Burma slowly expanded, its market towns increased in number and size, and an internal

market economy based on silver grew in sophistication. Finally there was a growth in the China trade, not by sea as in Vietnam but over caravan routes into Yunnan. By 1830 this trade may have come to represent a half to two-thirds the level of the maritime trade, and the Burmese had begun to wear Chinese silk on a considerable scale.[71]

If Lieberman is right, some of the arguments made about Japan in this period may be applicable to Burma's withdrawal from intense maritime interaction with the outside world. Unlike Japan, however, Burma did not increase its production faster than its population. Despite the much greater concentration on agriculture after 1600, crop failure and famine became increasingly frequent in the period 1661–1740, since royal authority no longer sufficed to ensure that irrigation channels were maintained.[72]

Ayutthaya's revenues undoubtedly suffered greatly in the 1640s from the ending of direct Japanese shipping and the drastic reduction in vessels from China. Nevertheless Siam had a late period of exceptional prosperity under King Narai (1656–88), whose capital remained a great independent port largely through his increasingly desperate balancing of one foreign group against another. As the other major Asian entrepôts of the long-distance trade—Makassar, Banten and Aceh—fell to Dutch pressure, Ayutthaya became ever more important for all the non-Dutch traders. After Makassar's first defeat by the Dutch-Bugis coalition in 1660, the English factors there recommended moving to Ayutthaya where the Portuguese and Muslims were already heading. 'Since the late wars in Cambodia have removed all commerce out of that barbarous hole, it is devolved thither [Siam] and the wealthiest of the Portuguese and Malayans [of Makassar] intend that as their station.'[73]

By 1664, when the VOC blockaded the capital to force on Narai a monopoly of his lucrative deerskin exports to Japan, the danger posed by the European companies was clear. Narai looked to both English and French as counterweights to Dutch monopoly pressure. Around 1680 it looked as though the English Company might provide this, especially when the talented Greek Constance Phaulkon (originally Constantin Hiérachy) rose rapidly in Siamese service as an English protégé. The English proved impossibly quarrelsome, however. The two factors trusted by Phaulkon and the Siamese—Samuel White and Richard Burnaby—left the service of the East India Company and became ever more embittered with its official representatives in Madras. In 1683 Burnaby was appointed Siamese Governor of Mergui, with White as Shahbandar (harbourmaster), as part of Phaulkon's plan to organize Narai's royal trade to India with European expertise. In 1687 Fort St George in Madras had grown so frustrated with its two errant sons that it declared war on Siam and abortively tried to seize Mergui.[74]

This left Phaulkon little alternative but Louis XIV of France, to whom he had offered Songkhla in 1685 as the price of support against the Dutch and English. In 1687 a French fleet of six warships sailed for Siam to back the new alliance. Unknown to the Thais, however, the French had decided they needed Bangkok

(the gateway to the capital and its river) rather than Songkhla, as well as a garrison at Mergui. Phaulkon was now exasperated with the English, and his marriage to a pious Japanese Catholic had reconciled him to the Roman Church and particularly to the hope of French Jesuits eventually to obtain the conversion of the King. He therefore persuaded Narai to accept these new French demands despite their unpopularity. With 600 French soldiers in the country and Phaulkon seemingly directing the King's policy, resentment against the Europeans reached unprecedented levels.

A reaction came in the form of a coup in April 1688, after the king became terminally ill (he died in July). Since Narai had no son, the succession was unclear, and the master of the royal elephants, Okphra Phetracha, profited from the resentment of the Buddhist monks and some of the urban populace against the foreigners in order to make his move. In April he seized the royal palace at Lopburi (Louvo) and had Phaulkon and the king's two brothers (and possible heirs) killed. When Narai died he succeeded without difficulty to the throne. The French garrisons were quickly besieged, and by the end of 1688 they were forced to leave, though with such ill grace that they provoked a further round of violence against foreigners, especially Catholic missionaries.[75]

The change of dynasty marked a reversal in foreign policy. For the remainder of the Ayutthaya period, Siam kept aloof from European entanglements. It did not deliberately seek isolation: the court still needed the revenues and goods derived from trade, and continued to employ Muslim Indians and (increasingly) Chinese to conduct it. Trade did continue, but, on the one hand, it declined drastically in overall volume, and, on the other, it shifted towards the east Asian pattern of overwhelming dependence on the Chinese junk trade.

After 1688 only the VOC remained among large European merchants in Siam, and it complained constantly about trade prospects, closing its office temporarily in 1705 and 1741.[76] Indian Muslim commerce continued the downward path on which it had begun in the 1680s. Even the Chinese branch of trade slumped during the reign of King Phetracha (1688–1703), before growing to new heights in the eighteenth century.[77] The tribute trade to China ceased entirely during that reign, and the *Tosen* (Chinese) junks plying between Ayutthaya and Nagasaki declined in number. Ayutthaya had become the principal Southeast Asian entrepôt for *Tosen* ships towards the end of Narai's reign, attracting shipping away from its rivals Cambodia and Patani. In the 1690s, however, the *Tosen* ships deserted Ayutthaya again for Cambodia on a large scale (see Table 8.3). The royal trade continued to use Chinese on voyages to the east and Indian Muslims to the west, but the king and his son managed to send only four ships to China (plus two to Manila and one to Tongking) in the period 1689–97, and none thereafter. In the last years of his reign Phetracha's ships concentrated on diplomatic ventures to Batavia (1702 and 1703) and Surat.[78]

The reality appears to be that even the general increase in Chinese shipping to Southeast Asia after 1684, when the Imperial ban on overseas trade was lifted, could not prevent a collapse in Ayutthaya's trade during Phetracha's reign. The

country was troubled by war and rebellion in Khorat, Patani and Ligor. Around 1700 the ruin of commerce was noted by a French missionary: 'the traders are reduced to great misery, foreigners no longer come: one has seen here this year only three or four Chinese junks with not much merchandise'.[79] The acute shortage of silver throughout this reign offered a clear indication of foreign trade decline. Although rice exports in the reign of King Thaisa (1709–33) lifted the Chinese junk trade to new heights, Ayutthaya was then an outpost of the Chinese trading network rather than a major international entrepôt. Even Chinese junk captains complained in this reign of a general impoverishment and decline in trade in the ports of mainland Southeast Asia.

How this retreat affected internal developments is more difficult to assess. The parallel with Burma and northern Vietnam is striking, however, in the way Siam progressively lost its ability to control its manpower during the first half of the eighteenth century, leading to weakness and factionalism at the centre.[80] The institutions of central control built up during the time of absolutism were not strong enough to survive the loss in external revenues—again in striking contrast to Japan.

The Archipelago states lacked the options of withdrawing from dependence on international trade or relying on the less dangerous Chinese junks. For the most part they had been created by trade and had few alternative resources to survive without it. When the most lucrative long-distance trade was lost to the VOC, the consequence was a diffusion of power among local dynasties.

In Java, the unified empire of Sultan Agung had fallen apart by the end of the tyrannical reign of Amangkurat I in 1677. Warfare was constant, with the VOC playing a large part, until in 1755 a permanent division of Java was imposed by a 'Dutch' peace. In Bali, the single kingdom with which foreigners dealt in the first half of the century, with its impressive capital at Gelgel, began to break up around 1650, and by 1700 there were eight distinct kingdoms with only a token recognition of the spiritual seniority of one of them— the Dewa Agung.[81] The rapid progress towards imposing the single political authority of Ternate on Maluku in 1580–1600, and that of Makassar on South Sulawesi in 1600–60, was thrown into reverse after the Dutch conquest of these capitals.

Myriad other river-ports had grown into substantial states though the trade revenue of the age of commerce—most notably Aceh, Palembang, Jambi, Banjarmasin, Brunei, Johor, Patani and Magindanao—but during the seventeenth century most of these lost the capacity to overawe their hinterlands and to attract or force shipping to their major port for the produce of the whole region. All continued as significant sultanates, with the aura of past glory helping to compensate for the lack of political control.

It would be a mistake to label this process 'refeudalization', or consider it a return to an older political bedrock. Most of these states had not existed before the age of commerce. They did exist after it. Identities had been permanently created. Even the constituent chiefdoms that in practice went their own way by 1700 were frequently the creations of the brief period when the harbour

principalities had flourished. Royal servants rewarded for service to the unified state in the form of a benefice of land turned themselves into hereditary chiefs, but continued to value that original appointment by a mighty king. All spoke the same language, literally and metaphorically; all acknowledged that they were the common heirs of a valid tradition which ought in an ideal world to be unified.

Aceh was the Archipelago state most successful in maintaining both its freedom of action and its status as a major international port. Its fate is exemplary, though to some extent extreme, because it had never been based on a single river system. The centralized system of government did for a time outlive the death of its founder, Iskandar Muda, in 1636. Up to the crippling series of Dutch blockades in the 1650s Aceh continued to channel west coast pepper and Perak tin to its capital, confirming its position as the premier port on the eastern side of the Bay of Bengal. By the end of that decade the VOC effectively controlled Perak's tin output, and in 1663 it also prised away the west coast Sumatran ports which had always been resentful of Acehnese control. Dobbin has shown that even those few commercial brokers of Minangkabau, who initially gained by throwing in their lot with the Dutch, had by the end of the century been destroyed by VOC monopoly and the reversion of the Minangkabau area to a more self-sufficient economy.[82] Finally, the east Sumatran state of Deli revolted against Aceh control in 1668.[83]

The reign of the first queen of Aceh, Taj al-Alam (1641–75), was notable for its peacefulness and prosperity in a troubled time, but it also witnessed the institutionalization of the diffusion of power that would mark the following two centuries. The war-leaders *(uleebalang)* whom Iskandar Muda had rewarded with grants of land in order to replace the power of established commercial aristocracy *(orangkaya)* became entrenched during Taj al-Alam's reign as local satraps. They now treasured their original temporary grants *(sarakata)* from Iskandar Muda as charters of hereditary entitlement. At the death of each of the subsequent queens, various combinations of these *uleebalangs* caused uproar in the capital as they attempted to advance their candidate for the throne—in opposition to the urban *orangkaya*'s preference for another queen. Their eventual success in restoring male rule in 1699 did nothing to improve stability. A major civil war for the succession rent the country in the 1720s, and when a measure of stability returned in the 1730s the authority of certain *uleebalang* 'to enthrone and dethrone sultans' had become established. The transition from a 'harbour autocracy' to a diffusion of power with some resemblances to feudalism had been accomplished in less than a century. The devolution of power was expressed culturally in the sudden flowering of literature in Acehnese (the oldest extant manuscript in which is dated 1663/4), whereas in its political and literary heyday Aceh expressed itself only in Malay, the language of commerce and Islam.[84]

This pattern was repeated in a number of states, not only in the Archipelago. The Kingdom of Lansang collapsed with exceptional suddenness after the death of its powerful ruler Surinyavongse in 1694. After a series of bitter succession

disputes, the kingdom was permanently divided by 1707 with dynasties at Luang Prabang, Vientiane and later Champassak. The internal unity of Arakan did not long outlast the loss of its trade in the 1660s. For the whole period 1684–1710 the throne was the plaything of the royal guard of foreign Muslim archers, and it never regained its internal coherence before the Burmese conquest of 1785. While Cambodia's history had been tortuous before 1660, it became more so when its river port declined into insignificance, internal division multiplied and consequent Siamese and Vietnamese intervention kept the country on its knees.

It is too easy to portray this shift in a negative light, as though strong centralized polities with wealthy rulers formed the only measure of wellbeing. On balance, the eighteenth century was more peaceful than the seventeenth in Southeast Asia, and the departure of its absolutist rulers did not cause universal regret. While the age of commerce had given rise to much cultural innovation and borrowing, the period of less profound interaction with the outside world which followed was marked by some of the most brilliant art and literature of Siam, Burma, Vietnam and Java. The important point is that a change of direction occurred in the seventeenth century that was not reversed until the twentieth.

NOTES

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14 Sources for Table 8.1 are given in Anthony Reid, *Southeast Asia in the Age of Commerce, II. Expansion and Crisis* (New Haven, 1993), p. 27.
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33 Zhongyang Qixiang Ju Qixiang Kexue Yanjiu Yuan [Central Metereological Agency, Centre for Research in Metereological Science], *Zhongguo jin wubai nian hanlao fenbutu ji [Yearly charts of wetness/dryness in China for the last five-hundred-year period]* (Beijing, 1981), pp. 323–6.

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9
A SEVENTEENTH-CENTURY ‘GENERAL CRISIS’ IN EAST ASIA?*

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Almost twenty years ago, several colleagues and I were discussing Geoffrey Parker and Lesley Smith’s then recently published volume on the ‘Seventeenth-Century Crisis’, when a specialist in Byzantine history told us that in his opinion at least, Parker, Smith, and the others who had contributed to their jointly edited work had got it all wrong. The really *important* ‘general crisis’ in pre-modern times, he believed, had occurred not in the seventeenth century but, rather, in the fourteenth.[1] As he went on to discuss the impact of climatic change, food shortages, epidemic disease, monetary fluctuations, and military operations on fourteenth-century Europe and the Middle East, I began to think about some of the great and terrible events that had occurred in East Asian history during that same century: the fall of the Kamakura Shogunate (1185–1330s) and the political turmoil of the Northern and Southern Dynasties *(Nambokuchō)* period (1336–92) in Japan; the economic and military disasters surrounding the fall of the Mongol Yüan dynasty (1279–1368) in China; and the food shortages, ‘Japanese pirate’ *(wakō)* raids, and civil wars that paved the way for the founding of the Yi dynasty (1392–1910) in Korea. In subsequent readings I added economic and political strife in fourteenth-century Southeast Asia, the decline of the Delhi Sultanate in India, the collapse of the Ilkhanate (1256–1335) in Persia, and the destructive rise of Timur (1336–1405) in Transoxania. Surely a case could be made, I came to think, for a ‘General Crisis of the Fourteenth Century’, one much broader in scope than even our Byzantine specialist had been considering.[2]

Five or six years later while doing some background reading on the economic history of late medieval Europe, I came across the following passage by the monetary historian John Munro:

> the most acute bullion famine, the true nadir of…monetary contraction [in the fifteenth century] was the 1440s and 1450s… From the late 1440s to the mid 1460s also appears to be the period of the most severe, acute depression in northern Europe during the fifteenth century.[3]

It struck me as more than a little curious that this ‘nadir of monetary contraction’ and ‘acute depression’ should have occurred when they did, for the middle

decades of the fifteenth century were also marked by severe monetary problems and perhaps even an economic 'depression' in much of East Asia. In 1449, for example, the Yi government in Korea, which had already encountered great difficulties supplying China with gold and particularly silver as part of its long-standing 'tributary' arrangements with the Ming dynasty, announced that it was abandoning efforts to mint copper coins for the Korean economy and returning to a currency system based primarily on mulberry and cotton cloth.[4] The Yi government did not resume its production of copper coins until well into the seventeenth century.

Copper coinage was a serious problem in the Ryūkyū Islands and in Japan during the middle decades of the fifteenth century as well. In 1459 ambassadors from the Ryūkyūs made an urgent though unsuccessful request for coins from the Chinese court, while in 1476 the recently retired Ashikaga shogun, Yoshimasa (1435–90), wrote the following to the authorities in Peking:

> Because of long-continued warfare in my humble state, all of the copper coins have been scattered and lost. The state coffers are completely empty. The land is laid waste. The people suffer from extreme poverty. We have absolutely neither means nor ways of protecting and saving them.[5]

The purpose of Yoshimasa's letter was to ask the Ming government to send an emergency shipment of copper coins in return for the goods that were being presented to China by the Japanese 'tribute mission' of 1476–77.[6] Emperor Hsien-tsung (r. 1465–87) reluctantly agreed to this request, but since Ming government mints had not been operating, at least not legally, since the 1430s,[7] supplies of copper coins (as well as supplies of gold and silver) in China were limited and the number of coins sent to Japan, many of which were probably counterfeit, was much smaller than the Japanese had requested.[8] The coins in question certainly did not solve Yoshimasa's or Japan's monetary problems. In 1480 he sent another plea to Peking: 'Nowhere in our land,' he wrote, 'can a single copper coin be found… We would appeal to the sympathy and mercy of our sage Emperor for a grant of …coins so that our urgent need may be met.'[9]

If we consider this rather dismal monetary history in the light of the social, economic, and political turmoil that was affecting much of East Asia at about that same time—drought, famine, disease, rebellion, factional battles, palace coups, and Mongol incursions in mid-fifteenth-century China; drought, famine, disease, regicide, and rebellion in mid-fifteenth-century Korea; drought, famine, disease, communal uprisings, shogunal assassination, and finally the Onin War (1467–77) in mid- and late-fifteenth-century Japan—one might well be tempted to argue for the existence of a 'general crisis' in the region. And if one goes on to add the poor harvests and monetary shortages that were then plaguing Europe, the Middle East, and South Asia, Anglo-French hostilities during the Hundred Years' War, civil wars in England and France, the bitter struggles for political control of the Iberian Peninsula, the fall of Constantinople to the Turks (1453),

the military campaigns of Ivan III (1462–1505) in Russia, and the bloody ouster of the Sayyids from Delhi by the Afghans (1451), then a case might also be made for a 'general crisis' in fifteenth-century world history, a crisis that once again touched most, if not all, of Eurasia.

But is it really useful to talk of a 'crisis' or a 'general crisis' virtually every time we turn around in our historical research?[10] Undoubtedly not. One problem, and this has been particularly true of recent studies of seventeenthcentury history, is that those terms have been both overused and used with an unfortunate lack of precision.[11] As Professor Niels Steensgaard remarked over twenty-five years ago concerning what was then the current scholarship on seventeenth-century Europe, 'It must be realized that nowadays the crisis is often merely an affirmation of the undisputable fact that something happened in the seventeenth century; the crisis has become a synonym for what historians concerned with other centuries call "history".'[12]

In 1975 Theodore Rabb echoed these concerns in his book *The Struggle for Stability in Early Modern Europe*. He wrote: 'Although many resort to the term "crisis",…hardly any two treatments agree in the meaning of the word, let alone its implications for an understanding of the sixteenth and seventeenth centuries.'[13] Several chapters later Rabb returned to this theme:

> Precise usage presumably requires conformity to the essential attributes of a word… One requirement, for example, is that a crisis must be short-lived. Maybe in historical perspective it can last for a little more than two decades, as it does in [Hugh] Trevor-Roper's formulation,[14] but it certainly cannot encompass a century, as it does for [Eric] Hobsbawm[15]…, or even longer, as in [Roland] Mousnier.[16] Nor can one evade the problem by suggesting that the 'general crisis' was composed of a succession of little 'crises,' as Mousnier implies. The phenomenon not only must be short-lived but also—and this is crucial—it ought to be *distinct*, both from what precedes and from what succeeds.[17]

Taking Professor Rabb's definition as a starting-point, then, was there a short-lived but distinctive 'crisis' in East Asia during the seventeenth century? I believe the answer is yes and I would argue that the crucial period, the period of what I will call 'general crisis'[18] in the region, falls between the early 1630s and the late 1640s. This is not to suggest that the peoples and governments of East Asia did not encounter serious problems at other times during the seventeenth century: as in other parts of the world, one can find evidence of food shortages, natural disasters, monetary fluctuations, or peasant unrest in most regions of East Asia in virtually every decade—indeed, in almost every year—before the modern period. Nevertheless, it will be the contention here that for the seventeenth century, at least, it was only during the 1630s and 1640s that economic, social, and political problems in various parts of East Asia coalesced in ways that would justify the use of the term 'general crisis' to describe them.

That the 1630s and 1640s were a particularly difficult time for China is easy enough to demonstrate. Although the Ming government had had its share of economic, political, and military upsets during the late sixteenth and early seventeenth centuries—renewed growth of eunuch power, waste and extravagance at court, factional battles in the bureaucracy, military reverses at the hands of the Japanese and the Manchus, and the White Lotus uprising of 1622, to cite just a few examples—on each occasion, the dynasty had been strong enough, wealthy enough, or sometimes simply lucky enough to regain its equilibrium. The Japanese were forced to give up Toyotomi Hideyoshi's (1536–98) dreams of conquering China in the late 1590s, the White Lotus rebels in Shantung and Pei-Chihli were thoroughly (if not all that quickly) defeated in 1622, the eunuch Wei Chung-hsien (1568–1627) and his supporters were removed from power in 1627, and the Manchus were not able to campaign south of the Great Wall until the very end of 1629.

During the early 1630s, however, the situation in the Ming empire began to deteriorate rapidly. Widespread drought and government economy measures caused rebel groups in the west and northwest of the country to grow in size and destructiveness, the Manchus resumed their attacks and were soon raiding deep into the North China Plain, and there were serious anti-government uprisings in Shantung, Pei-Chihli, and southern Liaotung in 1631, 1632, and 1633. By the mid-1630s the troubles had spread to central and eastern China. In September 1634, for example, there was a major upheaval in Nan-Chihli's T'ung-ch'eng County and shortly thereafter a region that just a few years earlier had been one of the most prosperous on earth, the lower Yangtze delta, began to experience severe difficulties. During the late 1630s and early 1640s those difficulties turned into disasters as drought, floods, locust attacks, famine, disease, monetary fluctuations, and, of course, military operations caused the deaths of millions of people and led to the fall of Peking to the Manchus in April 1644.

Nothing so dramatic or destructive happened in Japanese history during the middle decades of the seventeenth century. Nevertheless, it can be argued that Japan also experienced a serious economic crisis (and perhaps even a mild political one) during the late 1630s and early 1640s. To support this argument, and to attempt to meet the first of Professor Rabb's tests, that the period of crisis should be distinct from what precedes it, it seems necessary to look back at the relatively prosperous years in Japan during the late sixteenth and early seventeenth centuries. Japan specialists will perhaps recall the following passage by the late sixteenth-century writer Ōta Gyūichi:

> Ever since the advent of… Hideyoshi, gold and silver have gushed forth from the mountains and from the plains… In the old days, no one as much as laid an eye on gold. But in this age, there are none even among peasants and rustics…who have not handled gold and silver aplenty. Our Empire enjoys peace and prosperity; on the roads not one beggar nor outcast is to be seen.[19]

Ōta is overstating his case here,[20] but as George Elison has pointed out, Ōta's words were echoed in 1614 by an even more enthusiastic Miura Jōshin:

> What a marvelous age! Even peasants like me enjoy tranquillity and happiness, and there are wonderful things to be heard and seen…[We] … dwell in the land of bliss. If this is not a [Buddhist Paradise], then how is it that I and other men could meet with such great fortune?[21]

Only a few years later such statements are difficult if not impossible to find. Indeed, during the 1620s but especially during the 1630s, many of Japan's towns, villages, and even some of its cities began to experience serious economic difficulties. I have argued elsewhere that some of these difficulties were probably caused by the same climatic changes that were then severely affecting agricultural production in eastern China.[22] Nevertheless, peasants in many parts of Japan at this time were also suffering from the increasingly heavy taxes and services being demanded of them by their superiors. Stephen Vlastos quotes the following from a village headman in Nihonmatsu, who was protesting against corvée labour quotas in 1625:

> The…quota of our village…has been raised from 6 to 17 men, and this is causing the greatest difficulty to farmers. Every year some must sell their daughters just to keep from losing their land. Even so, 4 of the 11 farmers have been reduced to landless peasants.[23]

As Yamamoto Hirofumi has demonstrated in his recent and very welcome study of the so-called 'Kan'ei Period' (*Kan'ei jidai,* 1624–43) in Japanese history,[24] during the 1630s and early 1640s economic conditions in many parts of Japan grew even worse. For example, a series of climatic disasters and poor harvests from Kyushu to the northeast of the country[25] caused Japanese food prices to soar to unprecedented heights.[26] At the same time, although not always for the same reasons, commercial activity in Japan's greatest cities fell sharply. By the early 1640s not only had prominent traders in Nagasaki, Kyoto, and Osaka gone bankrupt,[27] but large numbers of merchants, artisans, and refugees from the countryside were starving to death in the streets of those and other cities.[28] With hunger and malnutrition went outbreaks of epidemic disease which are said to have killed hundreds of thousands of people throughout the country during the early 1640s.[29] Alarmed by sharp increases in violent crime[30] and by what some saw as the potential for widespread anti-government political activity,[31] the Tokugawa shogunate and local and regional leaders tightened domestic security and implemented a series of relief measures that were designed to relieve at least some of the pressure on what the shogunate's own inspectors called the 'exhausted peasantry'.[32]

During the early 1640s when the 'Great Famine of the Kan'ei Period' *(Kan'ei kikin)* reached its peak in Japan, literally millions of people were also dying of

malnutrition, starvation, and disease in China. In 1641, for example, dysentery and other diseases resulted in the deaths of many people in both Wu-hsien and Nanking. The following spring another epidemic devastated Wu-hsien and nearby areas in the Yangtze delta. In 1643 a major outbreak of disease struck Peking and in 1644 'an epidemic killed entire families [in Wu-chiang] and decimated inhabitants of whole streets, as survivors begged the gods for mercy'.[33] By the late 1630s and early 1640s, then, economic conditions in many parts of China and Japan had declined dramatically from the relatively prosperous years that both countries, and especially Japan, had known during the late sixteenth and early seventeenth centuries.

But what about other areas in East Asia at this time? Were the situations in the Philippines and in Korea such that we can speak with some confidence about a 'general crisis' throughout the region during the late 1630s and early 1640s? Although Manila had enjoyed considerable prosperity as late as the early 1630s,[34] by the late 1630s and early 1640s monetary and other problems, most of which were directly related to economic and political problems in the New World, had virtually paralysed the city's economy. That paralysis was an important factor in the massacre of more than 20,000 Chinese residents of the Philippines by the Spanish between November 1639 and March 1640. Nor were conditions in the islands helped by adverse weather conditions which, along with a shortage of Chinese farmers caused by the massacre of 1639–40, sharply reduced grain production. There was so little rainfall in southern Luzon in 1642, for example, that the rice crop could not even be sown, much less harvested.[35] Prolonged dry spells severely affected agricultural production in the Ryūkyū Islands and probably in Taiwan during these years as well.[36]

Whether there was a distinct and short-lived 'crisis' in mid-seventeenthcentury Korea is somewhat more difficult to substantiate. It is not that economic conditions on the Korean peninsula were good during that period. In fact, they were often terrible. Nevertheless, those conditions had deteriorated much earlier than those in most other parts of East Asia, in large measure because of Hideyoshi's destructive invasions of 1592 and 1597. One author has described the aftermath of those invasions as follows:

> The effects…on agriculture were…felt for many years. When the Japanese departed the amount of land under cultivation had been reduced to less than a third of the pre-war amount, and that of Kyongsang, the granary province, to less than a sixth. National revenues shrank in proportion, and the government for a time was reduced almost to impotence. Census and land registers had been destroyed, and it was at first impossible to assess taxes in any systematic way.[37]

The situation in Korea was further complicated by domestic political unrest during the mid-1620s and by the Manchu invasions of 1627 and 1636–7. The second of those invasions was a military and political humiliation for the

Koreans and resulted in the termination of the Yi dynasty's long-standing 'tributary relationship' with Ming China. The invasion also caused a good deal of destruction in the capital (Seoul) and in northwestern Korea, which added to the country's already serious economic problems.[38] Nor was the situation helped by the fact that, like much of the rest of East Asia, Korea experienced a series of climatic upsets during the mid-seventeenth century.[39] Although the economic impact of those upsets remains to be determined, it seems unlikely that southern Korea, which was the country's 'rice bowl', could have escaped the severe weather problems that afflicted eastern China and western Japan during the late 1630s and early 1640s.[40] In any event, like China and Japan, Korea also suffered from outbreaks of epidemic disease during this period, outbreaks that are likely to have further reduced economic activity in the country.[41]

Although it is clear that major economic problems existed throughout East Asia during the late 1630s and early 1640s, the underlying cause or causes of those problems are still a matter of some debate. In a recent article Professor Jack A. Goldstone has argued that in the case of seventeenth-century China at least (he does not deal specifically with other areas in East Asia), the answer can be found in a protracted and 'wide-ranging ecological crisis':

> Put simply, large agrarian states of this period were not equipped to deal with the impact of a steady growth of population that was in excess of the productivity of the land. The implications of this ecological shift went far beyond mere issues of poverty and population dislocation. Pressures on resources led to insistent inflation… As the tax systems of most early modern states were based on fixed rates of taxation on people or on land, tax revenues lagged behind prices. States thus had no choice but to seek to expand taxation… Yet attempts to enlarge state revenues met resistance from élites and the populace, and thus rarely succeeded… [T]he results were state bankruptcy and consequent loss of control of the military, élite-led…rebellion, and a combination of élite-mobilized and popular uprisings that manifested the breakdown of central authority.[42]

Although this argument seems to make some sense in the case of late Ming China, how well does it help to explain the problems that existed elsewhere in East Asia during the seventeenth century? For example, although Tokugawa Japan would surely fit Goldstone's definition of a 'large agrarian state'[43] that had experienced rapid population growth during the late sixteenth and early seventeenth centuries, recent research has suggested that far from lagging behind population growth, the amount of land under cultivation in Japan increased rapidly during this period. According to Kozo Yamamura, for example, 'it is probably safe to conclude that the amount of rice paddy in Japan more than doubled in the period from 1550 to 1650'.[44] At the same time, instead of a 'breakdown of central authority', it is generally thought that Japan witnessed significant increases in both central and regional power during the 1630s,

increases that led to what is sometimes called the 'completion of the *bakuhan* system'.[45]

Why, then, did Japan experience serious economic difficulties at almost precisely the same time the Ming dynasty was approaching its final demise? One answer may be that Professor Goldstone's protracted 'ecological crisis' was less important to economic and political developments in midseventeenth-century East Asia, including developments in China, than the climatic fluctuations mentioned earlier in this chapter. The cause or causes of these fluctuations remain to be determined, but one possibility is that they were due, at least in part, to short-term changes[46] in atmospheric circulation that significantly altered global wind and rainfall patterns.[47] These changes might help to explain why some areas in East Asia experienced very hot and dry conditions during the 1630s and 1640s while other areas suffered from unusual cold or dampness or both.[48]

Another possibility is that the economic impact of these changes in atmospheric circulation was compounded, particularly during the late 1630s and early 1640s, by increased amounts of volcanic dust in the atmosphere. Drawing on the pioneering work of H.H.Lamb, John D.Post has noted that

> Volcanic dust veils in the stratosphere will decrease the absorption of incoming radiation by reducing the transparency of the atmosphere, which in turn produces lower surface temperatures. The correlation of dense volcanic dust veils and intercontinental economic disturbances marked by high agricultural commodity prices can be documented… for [a number of] historical periods. A partial list of the years when the two phenomena were present together [1597–1601, 1638–41, 1693–98, 1709–12, 1766–71, 1783–86, 1811–18, 1835–41, 1845–50] resembles a catalog of the pre-industrial crisis years, not only in Europe but often in North America and East Asia.[49]

Although the dense volcanic dust veils recorded for 1638–41 cannot explain the crop failures that occurred throughout East Asia before and after that four-year period, the region as a whole did experience significant seismic activity and high food prices during the late 1630s and early 1640s.[50] Moreover, as Professor Lamb has pointed out, 'the four greatest famine-producing years of bad harvest since 1599 in the northern half of Japan were all years when great volcanic dust veils were present over the northern hemisphere'.[51] Given prevailing wind patterns, the same is likely to have been true for parts of northern China, Korea, and Manchuria as well.

Future research will undoubtedly reveal additional information on climatic change and its implications for the economic history of mid-seventeenthcentury East Asia,[52] but there are other matters which also deserve consideration here. One of these is money, a subject to which Professor Goldstone devotes considerable attention:

> It is sometimes suggested that in addition to internal factors a collapse in China's silver trade with Europe contributed to the Ming decline in the early seventeenth century… But however tempting it may be to link China's fortunes to Europe's via the silver trade, attributing Chinese economic difficulties to a fall in the number of European ships and bullion shipments reaching Manila and Macao is quite hyperbolic,[53] for it ignores the vast scale of the Chinese economy… Still, it may be suggested that because the European trade was in silver bullion it played a role far out of proportion to its scale in the economy, for it provided the crucial lubricant of economic activity—hard cash. This assumption, too, bends under the vast weight of the Chinese economy.[54]

Professor Goldstone goes on to supply some statistical evidence to support his contention that the Chinese economy was so large and the amount of silver circulating in the country was so plentiful as to make international trade and imports of foreign bullion 'hardly…noticeable in the over-all economy'.[55] However, some of Goldstone's evidence is questionable[56] while some of it is incorrect. In the latter category is his assertion that the value of silver relative to copper fell in China during the 1630s and 1640s: 'In 1630– 33,' he writes, 'it took from 236 to 250 copper coins to purchase 16.875 grams of silver, while in 1635–39 it took only 188.'[57] Unfortunately, the source on which Professor Goldstone bases this sentence provides these figures not for China but, rather, for Japan, where, for very good economic and political reasons of its own, the Tokugawa government was attempting to remove old copper coins from circulation during the 1630s. Under the circumstances, it is hardly surprising that Japan's silver-copper ratio narrowed at this time.[58]

In China, on the other hand, the value of silver relative to copper rose sharply in the late 1630s and early 1640s,[59] thus suggesting either that the amount of silver in circulation had been reduced, that copper had become more plentiful, or both. The last of these possibilities is probably the correct one. Counterfeiters of copper coins are known to have been active during this period, but it is also likely that silver was being withdrawn from circulation because of higher taxes and the increased hoarding of precious metals that traditionally accompanied economic and political instability in China.[60]

Also contributing to the widening in China's silver-copper ratio at this time was a decline in the country's silver imports. From a total of perhaps 65,000 kilograms in 1632, for example, silver exports from Manila to China fell dramatically during the mid- and late 1630s.[61] As was suggested earlier, it was the temporary collapse of Sino-Spanish trade at this time that led to the massacre of more than 20,000 Chinese in the Philippines between November 1639 and March 1640.[62] Nor was the economic situation in China helped by economic and political developments in Japan. After prohibiting Japanese nationals from trading abroad in 1635, the Tokugawa government expelled Portuguese traders

from Nagasaki in the summer of 1639, thus substantially reducing the amount of Japanese silver reaching Macao and Canton.[63]

Professor Goldstone acknowledges that silver shipments to China fell during the late 1630s and early 1640s, but says that 'the decline in China's silver trade appears to follow upon the decay of the Ming, rather than the other way around'.[64] It should be noted, however, that Sino-Spanish trade in Manila declined after 1635 not because Chinese goods were unavailable but rather because the Spanish did not receive sufficient silver from the New World to pay for them. As Philip IV of Spain wrote several years later:

> three-fourths of the merchandise which the citizens [of Manila] are accustomed to trade is pledged to the [Chinese], since the commerce has hitherto been sustained on credit alone; and as in…1636–37 no money went from [New Spain] from the goods which the citizens [of Manila] sent [to Acapulco in 1636], which the [Chinese] had sold on credit, they have not been able to satisfy these claims. For this reason the [Chinese] have gone away, and say that they are not willing to lose more than what they have lost.[65]

As will be seen below, however, the Chinese did not stay away for long. Despite the massacre of thousands of their fellow countrymen during the winter of 1639–40, silver from the New World was too important to the Chinese economy for Chinese merchants to give up on the Manila trade entirely.[66]

A somewhat similar situation soon developed in Japan. Despite deteriorating economic and political conditions all over East Asia, substantial quantities of Chinese silk, porcelain, and other goods continued to be shipped to Japan on Chinese and Dutch vessels during the late 1630s and early 1640s.[67] As early as 1639, however, Japanese merchants in Nagasaki, Osaka, Kyoto, and other cities were beginning to find it increasingly difficult to sell those and other 'luxury' goods at a profit. The result was what Professor Yamamoto has called the 'Kan'ei Panic' *(Kan'ei kyōkō)*.[68] In this 'panic' prices for Chinese raw silk and silk goods fell sharply, prominent merchants involved in the China trade were ruined financially, and large numbers of Japanese artisans were thrown out of work. As economic conditions in Japan worsened in the early 1640s, Japanese silver exports to China fell even further;[69] by 1642 they had apparently declined to their lowest level for many years.[70]

The precise impact of this decline (and of the depressed economic condi tions in Manila) on the fall of the Ming dynasty may be debatable,[71] but the importance of maritime trade and bullion imports to certain regions of China and certain sectors of the late Ming economy should not be underestimated. As a knowledgeable observer in Kwangtung province noted in 1647, the collapse of foreign trade in Canton after about 1640 had caused severe economic problems in the region:

> [local] merchants have experienced repeated difficulties, goods have not circulated, and trade has come to a [virtual] halt… It is therefore clear that when the people from Macao come [to Canton] to trade, Kwangtung prospers; when they do not come Kwangtung suffers.[72]

Students of the work of C.R.Boxer will be aware that what 'the people from Macao' (the Portuguese and their Chinese agents) took to Canton before 1640 was almost entirely Japanese and Spanish-American silver.[73] The same was true of many of the long-distance merchants who frequented the ports of Fukien and Chekiang provinces during these years as well. And, it might be noted, with a combined population of more than twenty million people and close commercial ties to other regions of China, the provinces of Kwangtung, Fukien, and Chekiang hardly played an insignificant role in the late Ming economy.

Earlier in this chapter I suggested that what I have called the 'general crisis' in mid-seventeenth-century East Asian history probably lasted from the 1630s to the late 1640s. What evidence is there that the crisis was coming to an end by the latter date? Although both climatic conditions and harvests in many parts of China apparently improved after the mid-1640s,[74] the Manchu conquest hardly ushered in an immediate return to the relatively prosperous times of the late sixteenth and early seventeenth centuries. In fact, as students of early Ch'ing dynasty (1644–1911) history will be aware, Professor Kishimoto Mio recently has referred to the period from the early 1660s to the mid1680s as a time of 'economic depression' in China.[75] Professor Kishimoto may be overstating her case, but it is certainly true that for much of that period, grain prices in many parts of China were low, manufactured goods sold poorly, and many people had great difficulties paying their rents and taxes.

As serious as these problems were, however, they do not seem to me to constitute a 'crisis' or a 'general crisis' comparable in scope or severity to the one that occurred in China and other parts of East Asia during the late 1630s and early 1640s. Nor were China's problems after 1660 necessarily related either to those of the earlier period or to events elsewhere in East Asia. It should be pointed out, for example, that Kishimoto does not see the early Ch'ing 'depression' as beginning until the early 1660s, and there is evidence to suggest that by the late 1640s the Chinese economy was beginning to recover from the disasters of just a few years earlier. Not only did grain and commodity prices return to something approaching normal during the late 1640s and early 1650s, but even foreign trade, which had suffered during the transition from Ming to Ch'ing rule, made a significant comeback. As one Ch'ing dynasty official later commented:

> I still remember the years around 1649 and 1650. At that time… foreign goods were in all the markets and commercial transactions among the people were often carried out with foreign silver coins. Because of this, [those coins] circulated in all the provinces and were found everywhere.[76]

This situation did not last, for during the late 1650s and early 1660s the Manchu authorities forcibly evacuated thousands of towns and villages along the southeastern coast in an attempt to eliminate the maritime trade on which enemies of the new regime had built a lucrative commercial empire. Although the evacuation policy was successful, it had a high economic cost. As the above quotation indicates, during the late 1640s and 1650s seaborne trade had once again brought substantial quantities of foreign silver to the coastal regions of China.[77] As in late Ming times, much of that silver then filtered into the interior where it helped to stimulate economic activity. Once the coastal evacuation policy had been implemented, however, the quantities of silver flowing to China again dropped sharply, thus contributing to Professor Kishimoto's 'depression'.[78] As soon as Ch'ing's prohibitions were implemented in the late 1650s and early 1660s, we are told, '[foreign] silver coins disappeared completely [from circulation]. This is clear proof that the source of wealth has been stopped up.'[79]

Unlike the late 1630s and early 1640s, however, there was no comparable 'stopping up of wealth' in Japan after 1660. Once the Kan'ei Famine had ended in 1643–44, Japanese agriculture regained its equilibrium, urban growth revived, the monetary system was improved, and foreign trade showed considerable strength. This is not to suggest that Japanese leaders did not encounter any economic problems during the second half of the seventeenth century. As Saitō Yōichi's chart of natural disasters clearly demonstrates, there were more than a few floods, droughts, and earthquakes with which Tokugawa and local officials had to cope during this period.[80] Without the volcanic dust veils that appear to have done such damage during the late 1630s, however, they and the Japanese people coped quite well. Indeed, unlike the unsettled conditions in China during the 1660s and 1670s, the general economic trend in Japan at that time seems to have been up.[81] Vlastos even argues that peasants in some parts of the country came to regard the years between the mid-1640s and the 1680s as a kind of 'golden age' in comparison with the terrible times they had experienced during the Kan'ei period.[82] At the same time merchants in Nagasaki, Kyoto, and other Japanese cities made up for shortages of Chinese goods caused by the Ch'ing dynasty's prohibitions on trade by importing raw silk and other items from Southeast Asia, Bengal, and even Persia.[83]

Although reliable statistical information on pre-modern Korea is difficult to find, the economic situation there also seems to have improved during the second half of the seventeenth century. Tax reforms implemented during the reign of King Hyojong in the 1650s are said to have been a considerable success, and between 1657 and 1669 the number of registered households in Seoul alone rose from 15,760 to 23,899. In Korea as a whole during that same twelve-year period, the total of registered households rose from 658,771 to 1,313,453.[84] These registration figures were of course compiled for tax purposes, but they suggest that despite occasional and sometimes quite serious economic problems—the terrible famine of 1671, to cite one example —the Yi government was beginning to

reassert some control after decades of political impotence. Another indication of the government's and the economy's gradual recovery in the second half of the century was the reintroduction for the first time in 200 years of officially minted copper coins, coins that are said to have stimulated commerce and been in widespread circulation by 1700.[85]

Given the Philippines' close connections with China during the late Ming period, it perhaps is not surprising that the economy of the islands also experienced some significant fluctuations after the Manchu conquest. Although the Chinese population in Manila recovered from virtually nothing the year after the 1639–40 massacre to an estimated 15,000 during the 1650s,[86] their numbers declined again during the 1660s and 1670s, in part because the Ch'ing maritime prohibitions made trading across the South China Sea very difficult.[87] The Philippines weathered the mid-century crisis, therefore, and even experienced a mild recovery in the 1650s, but the islands would not enjoy significant growth and prosperity again until China began an extended period of economic expansion during the 1680s.

In conclusion, it is difficult to accept the idea that East Asia *as a region* experienced a long-term crisis during the seventeenth century. The example of Tokugawa Japan alone would seem sufficient to discredit that notion. Nevertheless, if one follows Professor Rabb and looks for a short-term and distinctive crisis that affected virtually all of East Asia at the same time, then, for the reasons outlined above, I would point to the 1630s and 1640s. Those two decades also saw, it might be noted, one of the worst droughts ever recorded in South Asian history, serious crop failures in Europe and the Middle East, and political upheavals in Ireland, England, Scotland, France, Spain, Portugal, Italy, Bavaria, Austria, Sweden, Russia, Persia, and Turkey, to cite a few examples. Given these facts, perhaps the next task for those who are interested in the global dimensions of the 'seventeenth-century crisis' is to narrow their chronological focus. A better understanding of 'what went wrong' in various parts of the world between 1630 and 1650 will go a long way towards helping us understand not only some of the important issues in mid-seventeenth-century history but in pre-modern history in general. Indeed, we may discover what Frederick J.Teggart already knew a half century ago, that 'the comparison of histories is necessary for a comprehension of what has actually happened within the borders of any national state.'[88]

NOTES

* This is a revised version of a paper presented at the Association for Asian Studies Annual Meeting in Washington, DC, in March 1989. Published in *Modern Asian Studies*, XXIV (1990), pp. 661–82, and reprinted here by permission of Cambridge University Press and the author.

1 Unfortunately, the Byzantine specialist in question had not read the Parker and Smith volume carefully. At one point in their joint introduction, for example, Parker

and Smith specifically discuss the 'simultaneous unrest on a global scale' that had occurred during the fourteenth century. See the 'Introduction', p. 6.

2 For further discussions of the global nature of fourteenth-century economic and political problems, see Immanuel Wallerstein, *The Modern World-System: Capitalist Agriculture and the Origins of the European World-Economy in the Sixteenth Century* (New York, 1974), pp. 34–9; and Carlo M.Cipolla, *The Monetary Policy of Fourteenth-Century Florence* (Berkeley, Los Angeles, and London, 1982), pp. 1–29.

3 John Munro, 'Bullion flows and monetary contraction in late-medieval England and the Low Countries', in J.F.Richards (ed), *Precious Metals in the Later Medieval and Early Modern Worlds* (Durham, North Carolina, 1983), p. 121. For similar and related problems in Egypt at this time, see Boaz Shoshan, 'From silver to copper: monetary changes in fifteenth-century Egypt', *Studia Islamica*, 56 (1982) pp. 97–116; and Boaz Shoshan, 'Exchange rate policies in fifteenth-century Egypt', *Journal of the Economic and Social History of the Orient*, 29, pt. 1 (Feb. 1986) pp. 49–50.

4 Yi Taejin, 'Economic transformation and socio-political trends in sixteenthcentury East Asia' (paper presented at the Conference on the International History of Early Modern East Asia, Lihue, Hawaii, 3–9 January 1988), pp. 10–11.

5 Quoted in Delmer M.Brown, *Money Economy in Medieval Japan: A Study in the Use of Coins* (New Haven, CT, 1951), pp. 23–4. See also Yoshi S. Kuno, *Japanese Expansion on the Asiatic Continent*, 2 vols (Berkeley, Calif., 1937), 1, pp. 114–15, 286–91. It would be unwise to take Yoshimasa's words too literally here. Although it is clear that many areas of Japan, including the capital region, were experiencing severe problems at this time, other areas were taking advantage of the chaos at the top of the Japanese political world to move ahead both economically and politically.

6 On Sino-Japanese diplomatic and commercial relations at this time, see Tanaka Takeo, 'Japan's Relations with Overseas Countries', in John Whitney Hall and Toyoda Takeshi (eds), *Japan in the Muromachi Age* (Berkeley, Los Angeles, and London, 1977), pp. 168–71; and Wang Yi-t'ung, *Official Relations between China and Japan, 1368–1549* (Cambridge, Mass., 1953).

7 Ray Huang, *Taxation and Governmental Finance in Sixteenth-Century Ming China* (Cambridge, 1974), p. 75.

8 The small number of copper coins supplied by the Ming government to the tribute mission of 1468 had been a disappointment to the Japanese authorities as well. See L.Carrington Goodrich and Chaoying Fang (eds), *Dictionary of Ming Biography*, 2 vols (New York and London, 1976), 2, p. 1160.

9 Quoted in Brown, *Money Economy*, p. 24. See also Kuno, *Japanese Expansion*, 1: 290.

10 Colleagues in European history have informed me that there is now talk of a 'MidSixteenth-Century Crisis' in Britain and elsewhere.

11 As some readers may be aware, I have been one of the guilty parties in this. See my 'Some Observations on the "seventeenth-century crisis" in China and Japan', *Journal of Asian Studies*, 45, 2 (Feb. 1986), pp. 223–44; and my 'Ming observers of Ming decline: some Chinese views on the "seventeenth-century crisis" in comparative perspective', *Journal of the Royal Asiatic Society of Great Britain and Ireland*, no. 2 (1988), pp. 316–48. For further information on this subject as it

relates to East Asia, see S.A.M.Adshead, 'The seventeenth-century general crisis in China', *Asian Profile*, 1, 2 (1973), pp. 271–80; S.A.M.Adshead, *China in World History* (New York, 1988), pp. 207–9; Frederic Wakeman Jr, *The Great Enterprise: The Manchu Reconstruction of Imperial Order in Seventeenth-Century China*, 2 vols (Berkeley, Calif. and London, 1985), 1, pp. 1–20; and F.Wakeman Jr, 'China and the seventeenth-century crisis', *Late Imperial China*, 7, 1 (June 1986), pp. 1–26.

12 Niels Steensgaard, 'The seventeenth-century crisis', Chapter 2 in this volume, quotation from p. 33.

13 Theodore K.Rabb, *The Struggle for Stability in Early Modern Europe* (New York, 1975), p. vii. See also Ivo Schöffer, Chapter 4 in this volume.

14 This is a reference to Professor Trevor-Roper's article 'The general crisis of the seventeenth century', which first appeared in *Past and Present*, XVI (1959), but can also be found in Trevor Aston (ed.), *Crisis in Europe, 1560–1660* (Garden City, New York, 1967), pp. 63–102.

15 This is a reference to Professor Hobsbawm's article 'The crisis of the seventeenth century', which first appeared in *Past and Present*, V and VI (1954), but can also be found with an additional postscript in Aston (ed.), *Crisis in Europe*, pp. 5–62.

16 This is a reference to Professor Mousnier's *Les XVIe et XVIIe Siècles* (Paris, 1954), in which the 'general crisis of the seventeenth century' is seen to last from approximately 1598 to 1715.

17 Rabb, *The Struggle for Stability*, p. 29.

18 As was pointed out by an unidentified member of the audience at the Association for Asian Studies Annual Meeting in Washington, DC, at which this paper was first presented in March 1989, the term 'general crisis' can be defined in a number of ways. One of these ways is that the crisis in question affected virtually all aspects of a nation's or society's historical and cultural development, from economic, social, and political structures to religious beliefs and artistic expression. A second kind of 'general crisis' is more limited in scope and deals with severe economic and political problems over a wide geographical area. This chapter adopts the latter definition and concentrates on economic and political developments in China, Korea, Japan, and, to a lesser extent, in the Ryūkyū and Philippine Islands.

19 Ōta quoted in George Elison, 'The cross and the sword: patterns of Momoyama history', in Elison and Bardwell Smith (eds), *Warlords, Artists, and Commoners: Japan in the Sixteenth Century* (Honolulu, 1981), p. 55. See also Mary Elizabeth Berry, *Hideyoshi* (Cambridge, Mass., 1982), pp. 183–205; and Bernard Susser, The Toyotomi regime and the Daimyo', in Jeffrey P.Mass and William B.Hauser (eds), *The Bakufu in Japanese History* (Stanford, Calif. 1985), pp. 145–6.

20 Writing a few years after Ōta, the Jesuit priest João Rodrigues (1561–1634), who lived in Japan from 1577 to 1610, qualified his own glowing account of that country's prosperity in the late sixteenth century with the observation that although many had become rich during the reign of Hideyoshi, 'the ordinary folk and peasants were impoverished by the taxes they were obliged to pay'. See João Rodrigues, *This Island of Japon*, trans. and ed. Michael Cooper (Tokyo, 1973), p. 78.

21 Quoted in Elison, 'The cross and the sword', p. 55.

22 Atwell, 'Some observations', pp. 224–7.

23 Quoted in Stephen Vlastos, *Peasant Protests and Uprisings in Tokugawa Japan* (Berkeley, Calif. and London, 1986), p. 32.

24 Yamamoto Hirofumi, *Kan'ei jidai* (Tokyo, 1989), pp. 189–204. I am grateful to Professor Ronald P.Toby for bringing this work to my attention. For additional information on economic conditions in Japan during the Kan'ei period, see Yamaguchi Keiji and Sasaki Junnosuke, *Bakuhan taisei* (Tokyo, 1971), pp. 55–9; Asao Naohiro, *Sakoku* (Tokyo, 1975), pp. 368–76; Nagakura Tamotsu, 'Kan'ei no kikin to bakufu no taiō', in Rekishi kōron (ed.), *Edo jidai no kikin* (Tokyo, 1982), pp. 75–85; and Wakita Osamu (with James L.McClain), 'The commercial and urban policies of Oda Nobunaga and Toyotomi Hideyoshi', in John Whitney Hall, Nagahara Keiji, and Kozo Yamamura (eds), *Japan Before Tokugawa* (Princeton, NJ, 1981), pp. 244–5.

25 For chronological listings of these disasters, see Endō Motoo, *Kinsei seikatsushi nempyō* (Tokyo, 1982), pp. 58–64; Okajima Hatasu, *Nihon no sai-i shi* (Tokyo, 1967), pp. 49–50; and K.Takahashi and J.Nemoto, 'Relationship between climatic change, rice production, and population', in K.Takahashi and M.M. Yoshino (eds), *Climatic Change and Food Production* (Tokyo, 1978), p. 184.

26 For statistical information on these price increases, see Kyōto Daigaku kinsei bukkashi kenkyūkai (ed.), *Jugo-jushichi seiki ni okeru bukka hendō no kenkyū* (Kyoto, 1962), pp. 72–4; and Yamazaki Ryuzo, *Kinsei bukkashi kenkyū* (Tokyo, 1983), pp. 49–57.

27 Nagazumi Yoko, trans., *Hirado Oranda shokan nikki*, 4 vols. (Tokyo, 1970), 4, pp. 338–9; and Yamamoto, *Kan'ei jidai*, pp. 192–4.

28 See, for example, *Tokugawa jikki*, in Kuroita Katsumi (ed.), *(Shintei zōho) Kokushi taikei*, 62 vols (Tokyo, 1959–67), 40, pp. 258, 269–70, 272–5; and Asao, *Sakoku*, p. 368.

29 See Endō, *Kinsei seikatushi nempyō*, p. 62.

30 *Tokugawa jikki*, 40, pp. 159, 164, 213; and Yamamoto, *Kan'ei jidai*, pp. 194–6.

31 The Tokugawa authorities had legitimate concerns in this area. Only a few years earlier, in 1637–8, the shogunate had encountered serious difficulties putting down the so-called Shimabara Rebellion in western Kyushu. The origins of that rebellion were many and complex, but it seems clear that one of its causes was the terrible suffering caused by a series of poor harvests in the area between 1634 and 1637. See Irimoto Masao, *Shimabara no ran* (Tokyo, 1980), pp. 186–215; and Ivan Morris, *The Nobility of Failure: Tragic Heroes in the History of Japan* (Tokyo, 1982), p. 151.

32 *Tokugawa jikki*, 40, pp. 159, 164, 170, 188, 212–13, 226, 258, 269–75, and 280–1. See also Vlastos, *Peasant Protests*, p. 36.

33 Angela Ki Che Leung, 'Organized medicine in Ming-Qing China: state and private medical institutions in the Lower Yangzi region', *Late Imperial China*, 8, 1 (June 1987), pp. 141–2. See also Helen Dunstan, The Late Ming epidemics: a preliminary survey', *Ch'ing-shih wen-t'i*, 3, 3 (Dec. 1975), pp. 1–59. For a graphic description of economic conditions in the once prosperous city of Soochow during this period, see Yeh Shao-yüan, 'Ch'i-chen chi-wen lu', in *T'ung-shih* (Shanghai, 1911), *ts'e* 18, 2/10b.

34 See, for example, the comments in 'Letter from the Ecclesiastical Cabildo to Felipe IV', in E.H.Blair and J.A.Robertson (eds), *The Philippine Islands*, 55 vols (Cleveland, Ohio, 1903–09), 24, pp. 254–5.

35 'News from the Filipinas, 1640–42', in Blair and Robertson (eds), *The Philippine Islands*, 35, p. 123. It is of some interest here that abnormally dry conditions

were severely affecting agricultural production in Batavia during these years as well. See Leonard Blussé, *Strange Company: Chinese Settlers, Mestizo Women, and the Dutch in VOC Batavia* (Dordrecht, 1986), p. 64. See also the data presented by Anthony Reid at pp. 211–17 above.

36 Endō, *Kinsei seikatsushi nempyō*, p. 59; and H.H.Lamb, *Climate, History, and the Modern World* (London and New York, 1982), p. 227.

37 Han Woo-keun, *The History of Korea*, trans. Lee Kyung-shik (Honolulu, 1970), p. 273. See also Ki-baik Lee, *A New History of Korea*, trans. Edward W.Wagner with Edward J.Shultz (Cambridge, Mass., 1984), pp. 214–15; and Takashi Hatada, *A History of Korea*, trans., Warren W.Smith, Jr and Benjamin H.Hazard (Santa Barbara, 1969), pp. 75–81.

38 Ibid., pp. 80–81.

39 S.E.Moon, 'Climatic change in historical times in Korea', in Takahashi and Yoshino (eds), *Climatic Change and Food Production*, pp. 42–3. Professor Yi Taejin is currently conducting research in Korean archival material that suggests climatic change had a severe impact on the Korean economy during the mid-seventeenth century; see pp. 7–8 above.

40 Chung-yang ch'i-hsiang chü Ch'i-hsiang k'o-hsüeh yen-chiu yüan (ed.), *Chungkuo chin wu-pai nien han-lao fen-pu t'u-chi* (Peking, 1981), pp. 89–91; and M.M. Yoshino, 'Regionality of climatic change in monsoon Asia', in Takahashi and Yoshino (eds), *Climatic Change and Food Production*, pp. 332–3.

41 Homer B.Hulbert, *Hulbert's History of Korea*, ed. Clarence Norwood Weems, 2 vols (New York, 1962), 2, pp. 138–9.

42 Jack A.Goldstone, 'East and West in the seventeenth century: political crises in Stuart England, Ottoman Turkey, and Ming China', *Comparative Studies in Society and History*, 30 (1988), pp. 105–6.

43 It is generally thought that from a total of between 10 and 18 million in 1600, Japanese population rose to just above 30 million by 1725. See Gilbert Rozman, *Urban Networks in Ch'ing China and Tokugawa Japan* (Princeton, NJ, 1973), p. 77; and Ann Bowman Jannetta, *Epidemics and Mortality in Early Modern Japan* (Princeton, NJ, 1987), p. 29.

44 Kozo Yamamura, 'Returns on unification: economic growth in Japan, 1550–1650', in Hall, Nagahara, and Yamamura (eds), *Japan Before Tokugawa*, p. 334.

45 See, for example, Yamaguchi Keiji and Sasaki Junnosuke, *Bakuhan taisei* (Tokyo, 1971), pp. 55–9; and Kitajima Masamoto, Nakada Yasunao, and Murakami Tadashi, 'Kan'ei jidai to Bakuhan kokka', in Rekishi kōron (ed.), *Sakoku to Bakuhan kokka no seiritsu* (Tokyo, 1982), pp. 12–16. As Hall, Nagahara, and Yamamura note, the term *bakuhan* was 'coined by historians to refer to the Tokugawa system of government in which the shogunate [bakufu] constituted national authority while the daimyo exercised authority over their domains [han]'. See Hall, Nagahara, and Yamamura (eds), *Japan Before Tokugawa*, p. 373.

46 On the economic significance of short-term as opposed to long-term climatic fluctuations, see H.Flohn, 'Short-term climatic fluctuations and their economic role', in T.M.L.Wigley, M.J.Ingram, and G.Farmer (eds), *Climate and History: Studies in Past Climates and Their Impact on Man* (Cambridge, 1981), pp. 310–18; Lamb, *Climate, History, and the Modern World*, pp. 219–21; and the articles by C.J.E.Schuurmans and E.Rosini in A.Berger (ed.), *Climatic Variations and Variability: Facts and Theories* (Dordrecht, 1981).

47 These shifts in atmospheric circulation, in turn, may have been related to the longterm cooling trend in early-modern history that is sometimes known as the 'Little Ice Age'. For general discussions of this subject, see H.H.Lamb, *Climate: Present, Past, and Future, Volume 1: Fundamentals and Climate Now* (London, 1972), pp. 254–306; and H.H.Lamb, *Climate: Past, Present, and Future, Volume 2: Climatic History and the Future* (London and New York, 1977), pp. 461–73.

48 These climatic changes might also help to explain the devastating locust attacks which occurred in some parts of China during the late 1630s and 1640s. See Lamb, *Climate, History, and the Modern World*, p. 304, and the discussion of climate in the 'Introduction' at pp. 7–9 above.

49 John D.Post, *The Last Great Subsistence Crisis in the Western World* (Baltimore and London, 1977), p. xii. See also Lamb, *Climate: Past, Present, and Future, Volume 1*, pp. 410–35; and Emmanuel Le Roy Ladurie, 'History and Climate', in Peter Burke (ed.), *Economy and Society in Early Modern Europe* (New York, 1972), p. 151, where it is noted that 'between 1639 and 1643 and again between 1646 and 1650, France experienced a series of cold, wet summers which proved disastrous to grain production'.

50 Chang T'ing-yü (ed.), *Ming-shih*, 28 vols (Peking, 1974), 2, pp. 504–5; Endō, *Kinsei seikatsushi nempyō*, pp. 56–62; Yamazaki, *Kinsei bukkashi kenkyū*, pp. 49–57; and Nakayama Mio, 'Shindai zenki Kōnan no beika dōkō', *Shigaku zasshi*, 87, 9 (Sept. 1978), pp. 1–33. Students of the history of climate will be aware that the shifts in atmospheric circulation and increased seismic activity at this time may be connected to the fact that the mid-seventeenth century was also a time of very low sunspot activity. As John Gribbin has put it, 'In an era of declining sunspot activity…, the circulation of the atmosphere, prodded by the fingers of the solar wind, slips into very slow gear, producing…climatic anomalies…and slowing down the spinning top on which we live quite appreciably. This jolt produces a bigger than usual kick on the seismically active zones of the globe, producing a spate of earthquakes, volcanoes, and tidal waves.' See John Gribbin, *The Climatic Threat* (Glasgow, 1978), p. 152. On reduced sunspot activity in the seventeenth century, see John A.Eddy, 'The Maunder Minimum', Chapter 11 in this volume; and Wakeman, *The Great Enterprise*, 1, p. 7, n. 13.

51 Lamb, *Climate, History, and the Modern World*, p. 59.

52 Among the subjects which deserve further research in this area are changes in ocean currents and sea temperatures (including the so-called El Niño and La Niña phenomena in the Pacific) and their possible relationship to climatic change in premodern East Asia.

53 Here Goldstone is specifically criticizing the work of Pierre Chaunu. See Chaunu's 'Manille et Macao, face à la conjoncture des XVI et XVII siècles', *Annales: économies, sociétés, civilisations*, 17 (1962).

54 Goldstone, 'East and West', p. 115.

55 Ibid.

56 Drawing on the work of Joseph Needham and Ray Huang, for example, Goldstone asserts that individual Chinese merchants often carried upwards of 30,000 ounces of silver on business trips during the late sixteenth century. Given premodern transportation and security problems, one wonders how it would have been possible for individuals to move around the often dangerous late Ming countryside with

more than two tons of silver, an amount that would have bought food for thousands of people for an entire year.

57 Goldstone, 'East and West', p. 116.

58 Atwell, 'Some observations', p. 232. See also Kamiki Tetsuo and Yamamura Kozo, 'Silver mines and Sung coins—a monetary history of medieval and modern Japan in international perspective', in Richards (ed.), *Precious Metals in the Later Medieval and Early Modern Worlds,* p. 355; and Nihon Ginkō Chōsakyoku (ed.), *Nihon no kahei,* 11 vols (Tokyo, 1973), 2, pp. 127–31.

59 Yeh, 'Ch'i-chen chi-wen lu', *ts'e* 18, 2/6a; and Chang Lü-hsiang, 'T'ung-hsiang tsai-i chi', in Ch'en Heng-li (ed.), *Pu Nung-shu yen-chiu* (Peking, 1958), p. 325. Professor Goldstone is correct when he states that the value of silver relative to gold was falling during this same period. However, since gold was rarely used in commercial transactions in late imperial China, gold-silver ratios are of limited value in determining what was happening in the Ming monetary system. Even if the gold-silver ratio in China was 1:14 at the end of the Ming, that may simply mean that silver was fourteen times more plentiful than gold (or fourteen times less desirable than gold to some people). The ratio tells us nothing about the amount of silver (or gold) actually in circulation.

60 In part because grain prices in China were rising sharply during the late 1630s and early 1640s, Professor Goldstone suggests that silver was being dishoarded rather than hoarded. A much more likely explanation is that because of the natural disasters and food shortages mentioned earlier in this chapter, grain prices were rising even faster than the price of silver. Otherwise one would have to explain why silver prices for cotton, silk, mulberry leaves, and other non-food items were falling rather than rising during this supposed period of dishoarding. On the collapse of these non-food prices in late Ming times, see Mr Shen, 'Ch'i-huang chi-shih', in Ch'en (ed.), *Pu Nung-shu,* p. 290; and Mi Chu Wiens, 'Cotton textile production and rural social transformation in Early Modern China', *Chung-kuo wen-hua yen-chiu-so hsüeh-pao,* 7, 2 (1974): 525. For a recent study which deals in a general way with hoarding in early-modern China, see Charles P. Kindleberger, *Spenders and Hoarders: The World Distribution of Spanish American Silver, 1550–1750* (Singapore, 1989).

61 Antonio Alvarez de Abreu, 'Commerce between the Philippines and Nueva España', in Blair and Robertson, *The Philippine Islands,* 30, p. 86.

62 'Relation of the insurrection of the Chinese', in Blair and Robertson, *The Philippine Islands,* 29, pp. 208–58.

63 Dutch and Chinese merchants continued to export silver from Japan after 1639, but, with the exception of the years 1659–61, the amounts involved usually were far below the estimates made by leading Japanese authorities such as Kobata Atsushi and Iwao Seiichi for the early seventeenth century. See Atwell, 'Some observations', pp. 225, 231.

64 Goldstone, 'East and West', p. 116.

65 Philip IV quoted in Alvarez de Abreu, 'Commerce between the Philippines and Nueva España', in Blair and Robertson, *The Philippine Islands,* 30, p. 86. See also William Lytle Schurz, *The Manila Galleon* (New York, 1959), p. 91; and Adshead, *China in World History,* pp. 208–9.

66 It should be noted that economic difficulties had also led to the murder of thousands of Chinese in the Philippines in 1603. As was the case in the 1640s,

however, the Chinese soon returned to the islands in large numbers and, after a brief period of adjustment, trade with the Spanish resumed. See 'The Sangley insurrection', in Blair and Robertson, *The Philippine Islands,* 12, pp. 142–68.

67 As might be expected, Chinese and Dutch merchants were attempting to take economic advantage of the expulsion of the Portuguese from Nagasaki in 1639.

68 Yamamoto, *Kan'ei jidai,* pp. 192–4.

69 Nagazumi, *Hirado Oranda shōkan nikki,* 4, pp. 338–9; Murakami Naojirō, trans., *Nagasaki Oranda shōkan no nikki* (Tokyo, 1956), vol.1, p. 158; and Peter Pratt, *History of Japan: Compiled from the Records of the English East India Company,* ed. M.Paske-Smith, 2 vols (Kobe, 1931), 2, p. 293.

70 Oskar Nachod, *Die Beziehungen der Niederländischen Ostindischen Kompagnie zu Japan im siebzehnten Jahrhundert* (Leipzig, 1897), Beilage 63, p.CCVII.

71 Few people would quarrel with Professor Goldstone's statement that 'cumulating internal disorder, not an external interruption of silver supplies, seems to be the crucial cause of the Ming crisis.' Goldstone, 'East and West', p. 116.

72 T'ung Yang-chia quoted in *Wen-hsien ts'ung-pien* (Peking 1930–?), *chi* 24, p. 19b. See also Fu Lo-shu, *A Documentary Chronicle of Sino- Western Relations,* 2 vols (Tucson, 1966), 2, p. 7.

73 Professor Boxer's classic study of this subject is *The Great Ship from Amacon: Annals of Macao and the Old Japan Trade, 1555–1640* (Lisbon, 1959).

74 Students of climatic history will be aware that the long-term cooling trend in early-modern history sometimes known as the 'Little Ice Age' continued during the second half of the seventeenth century, causing farmers throughout the world to adjust to new growing conditions. Chinese farmers seem to have made this adjustment fairly successfully, because the country experienced a series of bumper grain harvests during the 1660s and 1670s. Of course, those harvests may have seemed better than they actually were because the overall demand for grain undoubtedly had fallen in response to the population losses that had occurred during the Ming-Ch'ing transition. The magnitude of those losses is a subject which deserves urgent attention.

75 Kishimoto Mio, 'The Kangxi depression and early Qing local markets', *Modern China,* 10, 2 (1984), pp. 227–56.

76 Mu T'ien-yen, 'Ch'ing k'ai hai-chin shu', in Ho Ch'ang-ling (ed.), *Huang Chao ching-shih wen-pien* (Taipei, n.d.), *ch.* 26/14b, p. 966. I am grateful to Professor Helen Dunstan for bringing this memorial to my attention. See also Ch'üan Hansheng, 'Ch'ing chung-yeh i-ch'ien Chiang-che mi chia ti pien-tung ch'ü-shih', in Ch'üan (ed.), *Chung-kuo ching-chi shih lun-ts'ung,* 2 vols (Hong Kong, 1972), 2, p. 514.

77 It is of course likely that as economic and political conditions in China improved during the late 1640s and early 1650s and business confidence rose, silver was also dishoarded.

78 In addition to the article by Kishimoto cited in note 75, see Hans Ulrich Vogel, 'Chinese central monetary policy, 1644–1800', *Late Imperial China,* 8, no. 2 (Dec. 1987), pp. 2–3.

79 Mu, 'Ch'ing k'ai hai-chin shu', *ch.* 26/14b, p. 966.

80 Saitō Yōichi, 'Edo jidai no saigai nempyō', in *Rekishi kōron,* no. 47 (October, 1979), pp. 10–20.

81 As in China, however, bumper grain harvests during this period sometimes helped to lower farm prices, thus creating severe hardships for some farmers.
82 Vlastos, *Peasant Protests,* pp. 40, 47. Other scholars would probably date the agricultural recovery to the 1650s. See, for example, Sasaki Junnosuke (with Ronald P.Toby), 'The changing rationale of daimyo control in the emergence of the bakuhan state', in Hall, Nagahara, and Kozo Yamamura (eds), *Japan Before Tokugawa,* pp. 286–91.
83 See the recent discussion of this subject in Om Prakash, *The Dutch East India Company and the Economy of Bengal, 1630–1720* (Princeton, NJ, 1985), pp. 118–41.
84 Han, *The History of Korea,* p. 309.
85 Lee, *A New History of Korea,* pp. 230–31.
86 Victor Purcell, *The Chinese in Southeast Asia* (London, 1965), p. 502.
87 Unlike the situation in the late 1630s, therefore, Sino-Spanish trade during the 1660s and 1670s was affected more by political decisions taken in China than by the availability of New World silver in Manila.
88 Frederick J.Teggart, *Rome and China: A Study of Correlations in Historical Events* (Berkeley, Calif., 1939), p. 243.

10
THE SEVENTEENTH-CENTURY CRISIS AND THE UNITY OF EURASIAN HISTORY*

Niels Steensgaard

The seventeenth-century crisis concept served research in European history well: it deepened and broadened our understanding of well-studied issues, even though we were far from agreeing on a definition of what the seventeenth-century crisis was or is. This is the kind of conceptual embarrassment which baffles our colleagues in the social sciences, but which the historian takes in his stride.

The concept served its purpose well, because the discussion of its content, or for that matter its very existence, revealed a number of general features in seventeenth-century European history, which used to be studied in national isolation. This not only meant new insights into the history of the continent, but also gave a new meaning to old issues in national historiographies. What might look like a unique drama on the national stage like the Fronde, the Revolt of the Catalans or the English Civil war, each with its own preconditions and actors and catastrophes, turned out to be so many variations on the same theme.

But there was and there still is no agreement as to what constituted the seventeenth-century crisis or what caused it or even whether it existed at all. To Trevor-Roper,[1] one of the early protagonists of the debate, the crisis of the seventeenth century was primarily the internal political conflicts in several European states in the middle decades of the seventeenth century, especially the 1640s, conflicts which marked the end of the parasitic Renaissance state and its court bureaucracy and inaugurated the Age of Enlightenment. As part of the background to the crisis, but not as an integrated part of the crisis, Trevor-Roper assumed a long-term depression in the European economy between the exuberant sixteenth century and the reassumed growth of the eighteenth century.

Eric Hobsbawm[2] accepted the same general economic trend from the sixteenth to the eighteenth century, but he saw the crisis of the seventeenth century in broad terms within the general framework of Marxist theory. By the early seventeenth century the Feudal economy had overextended itself, and the seventeenth century became a period of consolidation and realloca tion before the final break-through of the eighteenth century. A third, even broader interpretation of the seventeenth-century debate was present right from the beginning, formulated first by Roland Mousnier,[3] later presented most convincingly by Rabb.[4] In their view, the seventeenth-century crisis is not so much a phenomenon to be explained or interpreted as a phenomenon to be

described. It was a comprehensive crisis in European culture affecting not only economy and politics, but also art and attitudes, a deep-rooted insecurity and a loss of orientation, only surmounted after radical institutional changes and an intellectual reorientation exemplified by the development of a new scientific understanding of man and the world.

Nearly everybody concerned with the seventeenth-century crisis has assumed as part of their interpretation that there was a long-term regression or stagnation in the European economy from the early seventeenth to the early eighteenth century, exemplified in demographic development, prices, volume of international trade and agricultural and industrial production. This, incidentally, is the aspect of the crisis which more than any other has won universal recognition and become an established truth,[5] though the symptoms of crisis in the seventeenth-century European economy are not so easily accounted for as one might believe. It is disturbing, to say the least, that we seem to have stagnating grain prices and demographic strain *and* reduced yield ratios at the same time. Also, many statistical series quoted in support of the crisis theory, are more than offset by other statistical series. Very often a relocation or diffusion of production, rather than an absolute stagnation or decline, seems to be the most likely explanation of the strain felt in centres or branches of production.

I tried in 1970[6] to present a comprehensive theory for the seventeenthcentury crisis which linked the development of the European state in the seventeenth century to the symptoms of economic crisis. A sector which in no way can be said to show any signs of regression in the seventeenth century is government revenue and expenditure. The growth of the public sector, primarily in the size and provisions of armies, was effected by the transfer of income from low-income groups to the public sector. This transfer of income forms a plausible explanation of the contradictory symptoms of crisis in demography, agriculture and industry. If this is the case, the crisis of the seventeenth century was a crisis of distribution, not of production.

The debate on the seventeenth-century European crisis remained inconclusive; there is no consensus concerning its nature and causes. Still, the debate on the seventeenth-century European crisis usefully demonstrated, beyond doubt, that no national history in that period can be seen in isolation. However tenuous the links across the boundaries of the European states were in quantitative terms, Europe in the seventeenth century formed a *gestalt* of interdependent states and regions. The exact nature of this *gestalt* is a matter of argument. Immanuel Wallerstein thinks of it is a capitalist world economy, inclusive of Latin America already in the sixteenth century, but excluding Asia until 1750/1850. Wallerstein used the world *gestalt* polemically against a number of historians, particularly Frank Perlin, who have argued that the impact of European trade was more than an intrusion at the summit, dealing only in ‘splendid and trifling’ traffic, but that it reached into or was a significant factor in the structure of production and the political organization on the Indian subcontinent.[7] Now clearly the existence of some kind of *gestalt*, even if it is not a world economy, is the same problem as

the existence of some kind of seventeenth-century Eurasian crisis, only expressed in spatial terms. Even an inconclusive debate on a seventeenth-century Eurasian crisis may help us to see more clearly if Europe and Asia were two distinct worlds or if they in fact were only one continent (which is what they look like on the map) so that the distinction between the history of Asian and European societies was a matter more of 'epistemological status'[8] than of history. The problem of the seventeenth century crisis is as much a problem of geography as of chronology. Do the interrelations stop somewhere along a line drawn from the Baltic to the Mediterranean or perhaps the Black Sea, or do they include the whole of Eurasia? Recent research on Asia has drawn attention to several symptoms of crisis, such as political instability, possibly prompted by a recession in the international economy, widespread climactic deterioration and unusually severe harvest failures and famines. The whole question has especially concerned students of Indian economic history, who have been confronted with the sophistication of institutions and the potential for growth in seventeenth-century India, but also with a strong historiographical tradition denying structural changes in the centuries preceding the Industrial Revolution.[9] Wallerstein has, from his world-system perspective, turned polemically against such 'revisionism' especially as advocated by Perlin: 'Perlin wishes to see Western Europe, India, China, West Africa, and no doubt many other areas participating in some kind of *gestalt* (I say this vaguely because he himself hesitates to give it a name) within which developments occur.'[10] Wallerstein does not hesitate to give it a name: he calls it a world economy and as Asia by definition is outside the capitalistic world economy before 1750/1850, India cannot be inside. This may be true, if the argument is hinged upon hierarchically integrated production processes, though I am not too sure about that; but if we are looking for interrelations which do not presuppose a hierarchically organized world economy, the conclusion may be entirely different.

Very good arguments can be brought in to demonstrate that Europe and Asia shared the same conjuncture or the same *gestalt* without necessarily being hierarchically organized in the centuries between the Discoveries and the Industrial Revolution. As already emphasized by Braudel, the demographic development of China and Europe followed broadly the same trend between the fourteenth and the eighteenth centuries: a slow recuperation from the fourteenth-century plague; accelerating growth in the sixteenth century resumed in the eighteenth century after some regression; stagnation or reduced growth in the seventeenth century. Over the very long term, the same pattern may be detected in other regions of Asia. Though the evidence is insufficient to document the hesitations of the seventeenth century, the long-term population increase also included India (from around 145 millions *c.* 1600 to around 210 millions in *c.* 1800 according to the most recent estimate).[11] The growth rate of the population of Japan during the first half of the Tokugawa period was at an even higher level.

I should like to illustrate the complexity of the problems confronting us with an overlooked though significant example of the *conjoncture*, which seems to have tied Eurasia together in the sixteenth and seventeenth centuries: all fiscal systems in Eurasia from Japan in the East to England in the West seem to have been under unusual strain or undergoing important reforms in the sixteenth century. The comparative study of taxation is a speciality with few friends, perhaps for obvious reasons: not only is the subject unusually unappetizing, but the methodological problems, given the nature of the sources, are appalling. I do not aspire here to carry through a thorough comparison, only to bring forward enough evidence to show that we seem to be dealing with a Eurasian *conjoncture*, not with strains and reforms of the kind we come across in any century.

Ming China developed a system of taxation, which in complexity and attention to minute details surpassed the most fastidious expectations of the modern welfare state, though without access to the modern technology and administrative infrastructure that make an approximation of expectations and reality feasible. The burden of taxation was in principle not oppressive, either by imperial or by modern standards—3.5 to 7 per cent of the primary agricultural product—but the lack of administrative capacity and the strong vested interests which had to be accommodated, both in assessment and collection of the goods and services due to the empire, gave rise to considerable uncertainty. Taxpayers undoubtedly very often paid more than was their due, and the central government certainly controlled only a fraction of the total revenue.

The single-whip reform of the sixteenth century must be thought of as an attempt to rationalize the tax system in the interest of both the imperial authorities and the primary taxpayers. Its main content was the commutation into units of silver of a number of dues, labour services, commodity deliveries, and taxes habitually paid in kind. The design was recognized by the emperor in 1531, but the implementation was left to the discretion of local magistrates, who were expected to work in harmony with local interests. The reform was not entirely a success. Strong vested interests which only stood to lose from a simplification of the revenue system worked against it, and a shift from receiving deliveries in kind and labour services to accommodating all government needs from the open market raised innumerable problems. The reform was carried out only in part, so that the commutation of some dues, but not all, became another complication rather than a simplification.

The sixteenth-century endeavour to reform the Chinese system of taxation reached its culminating point under grand secretary Chang Chü-Cheng (1572–82) who succeeded in building up large reserves in the imperial treasuries, though more by audit and control—and probably helped by the high liquidity of the 1570s—than by real reforms. It is nevertheless significant that Chang Chü-Cheng from 1578 tried to carry out a national land survey, which, however, remained unfinished when the imperial government after Chang Chü-Cheng's death in 1582 curtailed his fiscal policy. In the event, no major reform of the

taxation system was completed and in spite of the seventeenthcentury breakdown and change of dynasty, the Chinese system of taxation remained in principle unchanged until the nineteenth century.[12]

The Japanese experience in the sixteenth century shows some remarkable parallels to the Chinese. In Japan, too, the reform of the system of taxation began at the local level, but here it was implemented by strong daimyos, who from a position of power formalized their control of the human and natural reserves of their lands by means of cadastral surveys and commutation of the assessment—not necessarily the payment—of taxes into money. These reforms were carried out on a hegemonic scale by Hideyoshi in the period 1583–98 until all the agricultural land of Japan had been surveyed and assessed in terms of rice. This gigantic administrative achievement had repercussions far beyond the confines of government finances: it became the basis of the Tokugawa shoguns' social and political control of daimyos, warriors and peasants and it was followed by what has been called an agricultural revolution, by the creation of a national market for rice and by sustained population increase.[13]

Also the revenue system of the Mughal Empire went through a process of revision and rationalization in the course of the sixteenth century. During the reign of Sher Shah (1540–45) average produce was fixed for each crop, making it possible to assess taxes on the basis of measurements of the sown area. This system was brought to its final form under Akbar, particularly in the years 1574/75–1595/96, when information on average yields and prices for each separate crop and each separate locality was collected, and the revenue rates were fixed in cash. In principle the revenue system made it possible for the cultivator to know the amount of tax to be paid, already when sowing his land, and for the central administration to know the revenue to be expected from every single village. In practice, however, the taxes only exceptionally went directly from the cultivator to the Mughal administration or its assignee; normally they would pass through middlemen, zamindars, headmen, contractors or others, whose rights were respected and whose services were indispensable.[14]

Russia may be included among the states which in the course of the second half of the sixteenth century established cadastral surveys aimed at more equitable and more efficient tax assessment, though the revenue system broke down during the years of trouble and was superseded by less ambitious, though certainly not less oppressive, forms of taxation in the seventeenth century.[15]

Finally Persia may possibly be included among the empires undergoing a tax reform in the late sixteenth century, though only in the sense of a more centralized control of the revenue to an extent which in the course of the seventeenth century is thought to have depleted the defence potentiality of the periphery.[16]

The revenue system of the Ottoman Empire did not undergo radical reforms in the sixteenth century, 'instead the groping leaders of the inflation era dealt in makeshift solutions and improvisations'.[17] A main feature was the gradual dismantling of the prebend system and the territorially based *sipahi* cavalry. The

enlarged imperial domain was turned over to tax farmers who paid in advance, thus acting as a kind of credit agency, though the insecurity of public credit at this stage may be illustrated by the fate of the notorious and immensely wealthy Greek, Michael Cantacuzenus, who was hanged from the gates of his own palace in Constantinople in 1578.[18] At the same time the Ottoman government made increasing use of its power to require extraordinary taxes from all of its subjects: 'this method of taxation, truly an exception at the beginning of the sixteenth century, had become customary by the century's end'.[19] The most striking example of the financial crisis of the Ottoman Empire, however, was the drastic reduction of the small silver coins, especially in the period 1584–1600.[20]

In the far West of the Eurasian continent the pattern is of course extremely complicated, but some recurring features may be discerned, confirming the overall impression that the sixteenth century was a century of fiscal upheaval all over the Eurasian continent. The principle that the prince should live off his own, i.e., that he should manage on the income from his domains and from specified regalia, was only slowly giving way to regular taxation of land and people. On the other hand, the European princes could make use of a wide variety of fiscal devices in bewildering competition with seignorial rights. The confiscation of Church land in all Protestant countries brought tremendous increases in the amount of land under the economic and seignorial control of the princes, while even in Spain and France the Church was made contributable to the government expenses. Of a more innovatory character was the increasing use of the sale of offices as a source of extraordinary income, especially in France and Spain. It was a peculiar device, highly repressive, of course, but at the same time giving the government access to the savings of the more prosperous subjects and giving the well-to-do the opportunity to make long-term investments in the state.

Short-term investment in the state in the form of anticipation or revenue farming had a very long history, of course, and seems by the sixteenth century to have been used everywhere in Eurasia, but it had hardly ever before been used to a greater extent than during the Habsburg-Valois struggles in the first half of the sixteenth century, the *Zeitalter der Fugger*. Also obligations became more binding than before, when the princes began to make use of an international money market, only partly under their control. As late as in 1527 Francis I could rid himself of a considerable part of his debt by having his chief financier, Semblançay, hanged and his property confiscated.[21] By the 1550s both France and Spain were unable to meet their obligations: 'In July 1556, for example, Philip II found at his accession that his Spanish revenues were pledged in full up to and including the year 1561.'[22] But the creditors were not hanged, they had their claims converted into longterm loans.

Long-term borrowing was the most original fiscal device introduced by European governments in the financial turmoil of the sixteenth century. The capital value of the long-term debt of the Spanish crown increased from 7 million ducats in 1556 to 85 million ducats in 1598.[23] Other European princes followed the Spanish example though on a more modest scale: in France Henri II collected

6,800,000 *livres* from the sale of *rentes* 1547–59, and in the early years of the religious wars (1559–74) investors more or less vohmtarily purchased *rentes* with a capital value of 25,900,000 *livres*[24] (6–7 million ducats), while the papacy seems to have borrowed 13 million crowns between 1526 and 1601.[25] Such figures will not impress the modern reader, but the Spanish long-term debt of 85 million may be compared to a Spanish state budget of a little more than 9 million ducats,[26] an annual import of silver from America in the peak decades 1581–1630 of 6.7 million ducats of which only a quarter constituted the public share,[27] or the share capital of the Dutch East India Company when founded in 1602 of less than 2 million ducats. The long-term debt may also be compared to its contrast, the cash treasury accumulated by the Great Mughal Akbar, containing at his death in 1605 gold, silver and copper coin estimated at 168 million rupees or *c.* 56 million ducats.[28]

Through borrowing, European sovereigns avoided the political conflicts attendant upon a curtailment of ancient rights or an increase in the fiscal burdens of the rural population. Eventually, however, the bill had to be paid and ways and means to extend the permanent ordinary revenue had to be found. The sixteenth century had seen the systematic development of government credit; in the seventeenth century tax increases could not be postponed any more, with the results which have been characterized as the seventeenth-century crisis: political conflict, dislocations in industry and trade, rural misery, but also the development of more efficient forms of taxation and a more careful balancing of the size of public debt in relation to national resources.

CONCLUSION

Let us return now to the seventeenth-century crisis. The evidence presented so far does not seem to confirm the hypothesis of a general Eurasian crisis, except possibly in the most narrow sense of a subsistence crisis of the 1630s and 1640s. On the other hand, the assumption that all Eurasia shared the same *conjoncture* was confirmed by the observation that all revenue systems in Eurasia seem to have passed through a phase of unusual strain and/or reform in the sixteenth century.

In my opinion, these observations may very well be linked together. I have argued earlier that the symptoms of economic and political crisis in seventeenth-century Europe may best be accounted for as the result of the transfer of income through taxation, diverting demand from the taxpayers to the public sector. In the states and empires of seventeenth-century Asia we find no such consistent tendency; the strain upon revenue systems in the sixteenth century was followed by a long period of stability. China might of course be brought forward as a contrary example; undoubtedly the economic costs of rebellion and war were extremely high, but the Chinese crisis of the seventeenth century was not brought about by a dynamic state regime encroaching upon customary rights, but

by a conservative regime which had not managed to overhaul the traditional revenue system.

So, the solution to the puzzle of the seventeenth-century crisis as a Eurasian phenomenon may be, that it did *not* affect Asia, but represented a new departure in the Western part of the continent.

NOTES

* This is a revised version of a paper presented at the Association for Asian Studies Annual Meeting in Washington, DC, in March 1989, first published in *Modern Asian Studies,* XXIV (1990), pp. 683–97. It is reprinted here by permission of Cambridge University Press and the author.

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11
THE ‘MAUNDER MINIMUM’
Sunspots and climate in the reign of Louis XIV*

John A.Eddy

It has long been thought that the sun is a constant star of regular and repeatable behaviour. Measurements of the radiative output, or solar constant, seem to justify the first assumption, and the record of periodicity in sunspot numbers is taken as evidence for the second. Both records, however, sample only the most recent history of the sun.

When we look at the longer record—of the last 1,000 years or so—we find indications that the sun may have undergone significant changes in behaviour, with possible terrestrial effects. Evidence for past solar change is largely of an indirect nature and should be subject to the most critical scrutiny Most accessible, and crucial to the basic issue of past constancy or inconstancy, is a long period in the late seventeenth and early eighteenth centuries when, some have claimed, almost no sunspots were seen. The period, from about 1645 until 1715, was pointed out in the 1890s by G.Spörer and E.W.Maunder. I have re-examined the contemporary reports and new evidence that has come to light since Maunder’s time and conclude that this seventy-year period was indeed a time when solar activity all but stopped. This behaviour is wholly unlike the modern behaviour of the sun which we have come to accept as normal, and the consequences for solar and terrestrial physics seem to me profound.

THE SUNSPOT CYCLE

Surely the best known features of the sun are sunspots and the regular cycle of solar activity, which waxes and wanes with a period of about eleven years. This cycle is most often shown as a plot of sunspot numbers (Figure 11.1)—a measure of the number of spots seen at one time on the visible half of the sun.[1] Sunspot numbers are recorded daily, but to illustrate long-term effects astronomers more often use the annual means, which smooth out the short term variations and average out the marked imprint of solar rotation.

There is as yet no complete physical explanation for the observed solar cycle. Modern theory attributes the periodic features of sunspots to the action of a solar dynamo in which convection and surface rotation interact to amplify and maintain an assumed initial magnetic field.[2] Dynamo models are successful in reproducing certain features of the eleven-year cycle, but with these models it is

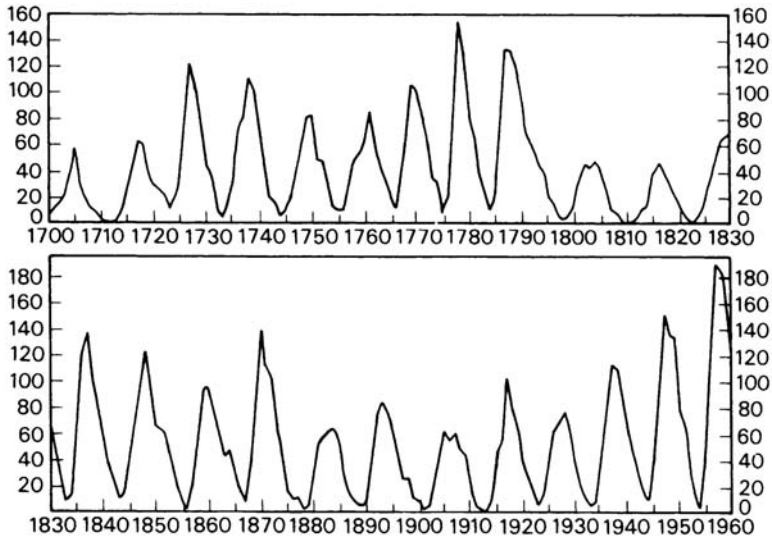

Figure 11.1 Annual mean sunspot number, R, from 1700 to 1960

Source: Waldmeier 1961

not as yet possible to explain the varying amplitudes of maxima and other long-term changes.

The annual mean sunspot number at a typical minimum in the eleven-year cycle is about six. During these minimum years there are stretches of days and weeks when no spots can be seen, but a monthly mean of zero is uncommon and there has been only one year (1810) in which the annual mean, to two-digit accuracy, was zero. In contrast, in the years around a sunspot maximum there is seldom a day when a number of spots cannot be seen, and often hundreds are present.

Past counts of sunspot number are readily available from the year 1700,[3] and workers in solar and terrestrial studies often use the record as though it were of uniform quality. In fact, it is not. Thus it is advisable, from time to time, to review the origin and pedigree of past sunspot numbers, and to recognize the uncertainty in much of the early record.

A BRIEF HISTORY

Dark spots were seen on the face of the sun at least as early as the fourth century BC,[4] but it was not until after the invention of the telescope, about 1610, that they were seen well enough to be associated with the sun itself. It would seem no credit to early astronomers that over 230 years elapsed between the telescopic ‘discovery’ of sunspots and the revelation of their now-obvious cyclic behaviour.

In 1843, Heinrich Schwabe, an amateur, published a brief paper reporting his observations of spots on the sun for the period 1826 to 1843 and pointing out an apparent period of about ten years between maxima in their number.[5]

Rudolf Wolf, director of the Observatory at Bern and later at Zurich, noticed Schwabe's paper and shortly after set out to test the result by extending the limited observations on which the ten-year cycle was based. In 1848 he organized a number of European observatories to record spots on a regular basis and by a standard scheme, thus inaugurating an international effort which continues today. Wolf also undertook a historical search and re-analysis of old data on sun in the literature and in observatory archives. More than half of the record of sunspot numbers in Figure 11.1, and all of it before 1848, is the result of Wolf's historical reconstruction. The most reliable part of the curve thus comes after 1848, when it is based on controlled observations. Wolf found descriptions and drawings of the sun which allowed him to reconstruct daily sunspot numbers thirty years into the past—to 1818— although, unlike the real-time data, they came from a thinner sample and with less certain corrections for observers and conditions. He was able to locate sufficient information on the more distant past to allow reconstructed 'monthly averages' of the sunspot number (that is, a minimum of one observation per month) to 1749, and approximate 'annual averages' from more scattered data to 1700.[6] The reliability of the curve, and especially of its absolute scale, may be graded into four epochs: reliable from 1848 on, good from 1818 to 1847, questionable from 1749 to 1817, and poor from 1700 to 1748.

Wolf collected data to extend the historical curve the final ninety years to the telescopic discovery of sunspots in 1610.[7] He published estimated dates of maxima and minima for 1610 to 1699, but not sunspot numbers. That he elected to discontinue sunspot numbers at 1700 may be significant: perhaps he felt he had reached the elastic limit of the sparse historical record at the even century mark; it could also be that at 1700 he ran into queer results. In this chapter I shall point out that the latter probably applies. It seems fair to assume that, once he had conflrmed and refined Schwabe's cycle, Wolf was biased towards demonstrating that the sunspot cycle persisted backward in time;[8] thus, when the cycle appeared to fade, especially in dim, historical data, he would have been inclined to quit the case and to call it proven. In any event we should be especially sceptical of the curve in its thinnest and oldest parts (1700 to 1748), and to question anew what happened before 1700.

Even though we are aware of the varying quality of the Wolf sunspot record, most of us probably take it as evidence of a truly continuous curve, much like the sample of a continuous wave form that we see on the screen of an oscilloscope. We assume that, just as Schwabe's seventeen-year sample was enough to reveal the cycle's existence, so the 260-year record in Figure 11.1 is adequate to establish its likely perpetuation to the future and extension through the past. Reconstructions of the solar cycle have been estimated from indirect data to the seventh century BC in the Spectrum of Time Project (STP) of D.J.Schove, but

these heroic efforts are of necessity based on far from continuous information and are built on the explicit assumption of a continued eleven-year cycle.[9,10,11,12] Recent insights into the physical basis for the sunspot cycle and its origin in the fluid, outer layers of the sun give us new cause to suspect that at least some of the features of the present sunspot cycle may be transitory. If we accept the solar dynamo, we must allow that any of its coupled forces could have changed enough in the past to alter or suspend the 'normal' solar cycle. Indeed, there is now evidence that solar rotation has varied significantly in historic time.[13]

THE 'PROLONGED SUNSPOT MINIMUM'

The possibility that sunspots sharply dropped in number before 1700 was pointed out rather clearly by two well-known solar astronomers in the late nineteenth century. In papers published in 1887 and 1889 the German astronomer Gustav Spörer called attention to a seventy-year period, ending about 1716, when there was a remarkable interruption in the ordinary course of the sunspot cycle and an almost total absence of spots.[14] Spörer was studying the distribution of sunspots with latitude and had found evidence that the numbers of spots in the northern and southern hemispheres of the sun were not always balanced. To check this observation he had consulted historical records, including Wolf's, and was surprised at what he found in the data of the seventeenth and early eighteenth centuries. Not long after, Spörer died. Meanwhile, E.W.Maunder, superintendent of the Solar Department, Greenwich Observatory, took up the case. In 1890 Maunder summarized Spörer's two papers for the Royal Astronomical Society and in 1894 gave a fuller account in an article entitled 'A Prolonged Sunspot Minimum'[15,16] In his second paper Maunder provided more details and pointed out that to acknowledge this unusual occurrence was to admit that the solar cycle and the sun itself had changed in historic time, and could again. He stressed that the reality of a 'prolonged sunspot minimum' had important implications not only for our understanding of the sun but also for studies of solar-terrestrial relations.

It is not obvious that anyone in solar physics listened. In any case, nearly thirty years later, at the age of 71, Maunder tried again with another paper of the same title on the same subject.[17] Included were quotations from a paper by Agnes Clerke who had claimed that during the 'prolonged sunspot minimum' there was also a marked dearth of aurorae.[18] Maunder offered as well the interesting conjecture that the long delay between the telescopic discovery of sunspots and Schwabe's discovery of the solar cycle may have been due in part to this temporary cessation of the solar cycle during a part of the interim.

In their five papers Spörer and Maunder made the following striking assertions: (1) that for a seventy-year period, from approximately 1645 to 1715, practically no sunspots were seen; (2) that for nearly half of this time (1672 to 1704) not a single spot was observed on the northern hemisphere of the sun; (3) that for sixty years, until 1705, no more than one sunspot *group* was seen on the

sun at a time; and (4) that during the entire seventy-year period no more than 'a handful' of spots were observed and that these were mostly single spots and at low solar latitudes, lasting for a single rotation or less; moreover, the total number of spots observed from 1645 to 1715 was less than what we see in a single active year under normal conditions.

Maunder supported these claims with quotations from the scientific literature of the period in question. The editor of the *Philosophical Transactions of the Royal Society*, in reporting the discovery of a sunspot in 1671 (in the middle of the 'prolonged sunspot minimum'), had written that 'at Paris the Excellent Signior Cassini hath lately detected again Spots in the Sun, of which none have been seen these many years that we know of'.[19] (Following this, the editor went on to describe the last sunspot seen, eleven years before, for those who might have forgotten what one looked like.)

Cassini' s own description of his 1671 sighting reads as follows: 'it is now about 20 years since astronomers have seen any considerable spots on the sun, though before that time, since the invention of the telescopes they have from time to time observed them.'[20] Cassini also reported that another French astronomer, Picard, 'was pleased at the discovery of a sunspot since it was ten whole years since he had seen one, no matter how great the care which he had taken from time to time to watch for them'.[21] And when the Astronomer Royal, Flamsteed, sighted a spot on the sun at Greenwich in 1684, he reported that '[t] hese appearances, however frequent in the days of *Scheiner* and *Galileo*, have been so rare of late that this is the only one I have seen in his face since *December* 1676'.[22]

Maunder did not have to look hard to find support for the strange case, for an absence of sunspots in the latter part of the seventeenth century had been matter-of-factly reported in astronomy books written before Schwabe's discovery of the cycle.[23] William Herschel had mentioned it in 1801.[24] Herschel's source of information was Lalande's three-volume opus, *Astronomie*, of 1792, in which dates and details are given of the anomalous absence of sunspots, including some of the quotations that Maunder later used.[25] Thus, neither Maunder nor Spörer had 'discovered' the 'prolonged sunspot minimum'. These authors, like myself, were simply pointing back to an overlooked and possibly important phenomenon which in its time had not seemed unusual but which looms large in retrospect.

QUESTIONS

Maunder's assessment of the significance of the 'prolonged sunspot minimum' was probably not an exaggeration. If solar activity really ceased or sank to near-zero level, it places a restrictive boundary condition on physical explanations of the solar cycle and suggests that a workable mechanism for solar activity must be capable of starting, and maybe stopping, in periods of tens of years. It labels sunspots as possibly transitory characteristics of the sun and, by association, also flares, active prominences, and perhaps the structured corona. One of the

enigmas in historical studies of the sun is the long delay in the naked-eye discovery of the chromosphere[26] and the lack of any ancient descriptions of coronal streamers at eclipse.[27,28] It may be more than curious coincidence that the discovery of the chromosphere (1706), the first description of the structured corona (1715), and a lasting, tenfold jump in the number of recorded aurorae (1716) all came at the end of the 'Maunder Minimum', when, it seems, the solar cycle resumed, or possibly began, its modern course. If Maunder's 'prolonged sunspot minimum' really happened, it provides damning evidence[29] in the protracted debate over the production of sunspots by planetary gravitational tides, for through the years between 1645 and 1715 the nine planets were, as always, in their orbits. Finally, as Maunder stressed, this apparent anomaly in the sun's history, if real, offers a singularly valuable test period for studies of the connection between solar activity and terrestrial weather. If the 'Maunder Minimum' really occurred, it may define a minimum of a long-term envelope of solar activity which could be more important for terrestrial implications than the eleven-year modulation that has for so long occupied attention in solar-terrestrial studies.[30]

It seems worthwhile to open, once again, the case of the missing sunspots, for it was never really solved. All the early work was based almost entirely on the same piece of evidence: the paucity of sunspot reports in the limited literature of the day. Spörer's original papers and Maunder's expansions of them leaned heavily on a lack of evidence in archival records and journals, and on contemporary statements that it had been a long time between sunspot reports. But in the words of a modern astronomer, absence of evidence is not evidence of absence.[31] How good were the observers in the seventeenth century, and how good the observing techniques? How constant a watch was kept? How many spots were missing, and when? New evidence has come to light in the fifty years since Maunder's time: we now have better catalogues of historical aurorae, compilations of sunspot observations made in the Orient, a fuller understanding of tree-ring records, and a new tool in atmospheric isotopes as tracers of past solar activity New understanding of the sun since Maunder's day can sharpen our assessment of the facts in the case: we now know the relationship of sunspots to solar magnetic fields and something of the relation of magnetic fields to the corona, and can thus examine more critically the evidence from total solar eclipses during the time.

SOLAR OBSERVATIONS IN THE SEVENTEENTH CENTURY

History has left an uncanny mnemonic for the dates of the 'Maunder Minimum': the reign of Louis XIV, *le Roi Soleil*—1643 through 1715. This was also the time of Milton and Newton; by 1642 Brahe, Kepler and Galileo were gone. Astronomical telescopes were in common use and were produced commercially; they featured innovations and important improvements over the original

miniature models which in 1612 had sufficed to distinguish umbrae and penumbrae in sunspots and by 1625 had been used to find the solar faculae. During the 'Maunder Minimum' the Greenwich and Paris observatories were founded, and Newton produced the reflecting telescope; it was also the age of the long, suspended, and aerial telescopes with focal lengths that stretched to 60 metres and apertures of 20 centimetres and more.[32] The more usual telescopes turned on the sun had focal lengths of 2 to 4 metres and apertures of 5 to 10 centimetres, which would describe most solar telescopes used in the eighteenth and nineteenth centuries as well. To observe sunspots then, as today, one projected the solar image on a white screen placed at a proper distance behind the eyepiece. The image scale was adequate to permit one to see and sketch not only spots of all sizes but their features and their differences; observers recorded details of white-light faculae, penumbral filaments, satellite sunspots, and most of the observational detail known of sunspots today.

During the 'Maunder Minimum' the same astronomers who observed the sun discovered the first division in Saturn's ring (in 1675) and found five of Saturn's satellites (1655 to 1684); the former discovery attests to an effective resolution of almost 1 arc second and the latter to an acuity to distinguish an 11th-magnitude object less than 40 arc seconds from the bright limb of the planet. During the seventeenth century astronomers observed seven transits of Venus and Mercury, which implies a certain thoroughness and a knowledge of other spots on the sun at the time. Römer determined the velocity of light (1675) from precise observations of the orbits of Jupiter's satellites. During the same century at least fifty-three eclipses of the sun—partial, annular or total—were observed, including some in Asia and the Americas. It is significant that not one solar eclipse that passed through Europe was missed.[33,34]

Active astronomers of the time included Flamsteed, Derham, Hooke and Halley in England, both of the Huyghens in Holland, Hevelius in Poland, Römer in Denmark, the Cassinis, Gassendi, de la Hire and Boulliau in France, Grimaldi and Riccioli in Italy, and Weigel and von Wurzelbau in Germany, to name but a few. And astronomers of that era were generous in their definition of astronomy and still included the sun among objects of respectable interest. During the years when the Cassinis were pursuing their investigations of Saturn in Paris, they also wrote scientific articles on their observations of the sun and sunspots.[35] In 1630 Christopher Scheiner published a massive book, the *Rosa Ursina*, on sunspots and faculae and methods of observing them,[36] and Hevelius produced in 1647 a detailed appendix on sunspots and a chapter on solar observation in his *Selenographia*.[37]

In 1801 William Herschel commented that instrumental and observational shortcomings could explain most of the sunspot dearth between 1650 and 1713, and that, had more modern equipment been turned on the sun, many more spots would have been found;[38] but we have little cause to think that he had looked very far into the matter, which then seemed of minor import, long before the discovery of the sunspot cycle. Maunder did not cite Herschel's dissenting view,

but trumped it anyway, with a quotation from the more contemporary English astronomer William Derham, who in 1711 had given his view on whether observers of the time could have missed the spots:

> There are doubtless great intervals sometimes when the Sun is free, as between the years 1660 and 1671, 1676 and 1684, in which time, Spots could hardly escape the sight of so many Observers of the Sun, as were then perpetually peeping upon him with their Telescopes in *England, France, Germany, Italy,* and all the World over.[39]

It seems clear that on this question Derham was right and Herschel wrong and that during the period of the 'Maunder Minimum' astronomers had the instruments, the knowledge, and the ability to recognize the presence or absence of even small spots on the sun. And I might add that it does not take much of a telescope to see a sunspot.

Was a continuous watch kept on the sun? This is quite another question, and one for which direct evidence is lacking. Scheiner (1575–1650) and Hevelius (1611–87) for at least a number of years made daily drawings of the sun and sunspots, but we cannot assume that this dutiful practice was continued by successors without interruption for seventy years. There were no organized or co-operative efforts, so far as we know, to keep a continuous diary of the sun, as is done today. But the motives of astronomers, then and now, are much the same: when a surprising dearth of sunspots was reported, as it was on repeated occasions during the span, we can expect that it would have inspired a renewed search to find some. In this respect it is significant that new sunspots were reported in the scientific literature as 'discoveries', and that the sighting of a new spot or spot group was cause for the writihg of a paper.[40] This practice, were it followed today by even a few owners of 5-centimetre refractors, would produce an intolerable glut of manuscripts in the minimum years of the sunspot cycle and an avalanche in the years of maximum.

Comparisons with the present time are dangerous: towards the end of the seventeenth century the first learned Societies were founded and the first journals came into existence. These journals were limited in number and scope and restricted in authorship, and in that time bore little resemblance to the scientific periodicals we read and rely on for thorough coverage today. Absence of evidence may be a limited clue in such circumstances, as may uncontested and possibly unrefereed reports. Moreover, prevailing ideas of what something *is* influence how it is observed and reported. Sunspots were not thought to be what we know they are today. The original theological opposition to spots on the sun had been assuaged long before 1645, but, throughout the period of the 'Maunder Minimum' and until Wilson's observations in 1774,[41] a prevalent concept of sunspots was that they were clouds on the sun, and who keeps a diary of clouds? Finally, we can suspect that sunspots, like all else in science, went in and out of vogue as objects of intense interest. After the initial surge of telescopic

investigation, sunspots may have drifted into the doldrums of current science. If this is so, Scheiner's massive tome may have been in part to blame: the *Rosa Ursina* must have been considered a bore by even the verbose standards of its day, and it may have smothered initiative for a time.[42,43]

AURORAE

Records of occurrence of the aurora borealis and aurora australis offer an independent check on past solar activity since there is a well-established correlation between sunspot number and the number of nights when aurorae are seen. The physical connection is indirect: auroral displays are produced when charged particles from the sun interact with the earth's magnetic field, resulting in particle accelerations and collisions with air molecules in our upper atmosphere. Aurorae register, therefore, those particle-producing events on the sun (such as flares and prominence eruptions) which happen to direct their streams towards the earth. Since these events arise in active regions on the sun, where there are also sunspots, we find a strong positive correlation between reported numbers of the two phenomena.

Aurorae are especially valuable as historical indicators of solar activity since they are spectacular and easily seen, require no telescopic apparatus, and are visible for hours over wide geographic areas. They have been recorded far back in history as objects of awe and wonder.

An increase in the number of reported aurorae inevitably follows a major increase in solar activity, and a drop in their number can generally be associated with the persistence of low numbers of sunspots, with certain reservations. As with sunspots, aurorae will not be seen unless the sky is reasonably clear, and an absence of either on any date in historical records could be due simply to foul weather. For the period of our interest we can exclude the possibility of years or decades of persistent continental overcast, since this would constitute a significant meteorological anomaly which would certainly have been noted in weather lore or cited by astronomers of the day.[44]

In fact, the period between 1645 and 1715 was characterized by a marked absence of aurorae, as was first pointed out by Clerke. 'There is,' she wrote, '… strong, although indirect evidence that the "prolonged sunspot minimum" was attended by a profound magnetic calm.'[45] Historical aurora catalogues[46,47] confirm her assessment that there were extremely few aurorae reported during the years of the 'Maunder Minimum'. Far fewer were recorded than in either the seventy years preceding or following.

Auroral occurrence is a strong function of latitude, or more specifically of distance from the geomagnetic poles. Analyses of auroral counts in the modern era[48] lead us to expect a display almost every night in the northern 'auroral zone'—a band of geomagnetic latitude that includes northern Siberia, far-northern Scandinavia, Iceland, Greenland and the northern halves of Canada and Alaska. But this region is also an area of sparse historical record for the

seventeenth century, and it should probably be excluded from consideration for the present purpose. In a more populous band just south of this zone—which includes Sweden, Norway and Scotland—we expect aurorae on 25 to about 200 nights per average year, the higher number at higher latitude. Progressively fewer are expected as we move south. For most of England, including the London area, we expect to see an average of 5 to 10 aurorae per year, or roughly 500 in seventy 'normal' years. In Paris we can expect about 350 in the same period, and in Italy perhaps 50. From England, France, Germany, Denmark and Poland, where astronomers were active during the 'Maunder Minimum', we might have expected reports of 300 to 1,000 auroral nights, by the statistics of today. Fritz's historical catalogue[49] lists only 77 aurorae for the entire world during the years from 1645 to 1715, and 20 of these were reported in a brief active interval, from 1707 to 1708, when sunspots were also seen. In thirty-seven of the years of the 'Maunder Minimum' not a single aurora was reported anywhere. Practically all reported aurorae were from the northern part of Europe: Norway, Sweden, Germany and Poland. For sixty-three years of the 'Maunder Minimum', from 1645 until 1708, not one was reported in London. The next, on 15 March 1716, moved the astronomer Edmund Halley to describe and explain it in a paper that is now classic.[50] He was then 60 years old and had never seen an aurora before, although he was an assiduous observer of the sky and had long wanted to observe one.

The auroral picture, which seems clear at first glance, is muddied by subjectivity and by the obscurity of indirect facts from long ago. Historical catalogues cannot record aurorae but only reports of aurorae. Clerke did not mention that auroral counts from all centuries before the 18th are very low by modern standards. The seventy-seven events noted during the 'Maunder Minimum' actually exceed the number recorded in any preceding century except the sixteenth, for which there are 161 in Fritz's catalogue. By contrast, 6,126 were reported in the eighteenth century and about as many in the nineteenth century.[51]

The really striking feature of the historical record of aurorae (Figure 11.2) is not so much the drop during the 'Maunder Minimum' but an apparent 'auroral turn-on' which commenced in the middle sixteenth century and surged upward dramatically after 1716. Were the historical record of uniform quality (and it is not), this apparent 'switching on' of the northern (and southern) lights would loom as the most significant fact of recent solarterrestrial history. In truth, it must in part at least reflect the general curve of learning which probably holds for all of life in northern Europe at the time. The Renaissance came to auroral latitudes later than to the Mediterranean, and the envelope we see in Figure 11.2 may be but its shadow. The effect is large, however, and a part of it could well represent a real change in the occurrence of aurorae on the earth, and, by implication, a change in the behaviour of the sun. It is important that auroral reports do not increase monotonically with time as a learning curve might imply; the number reported rose in the ninth to twelfth centuries and then fell off.

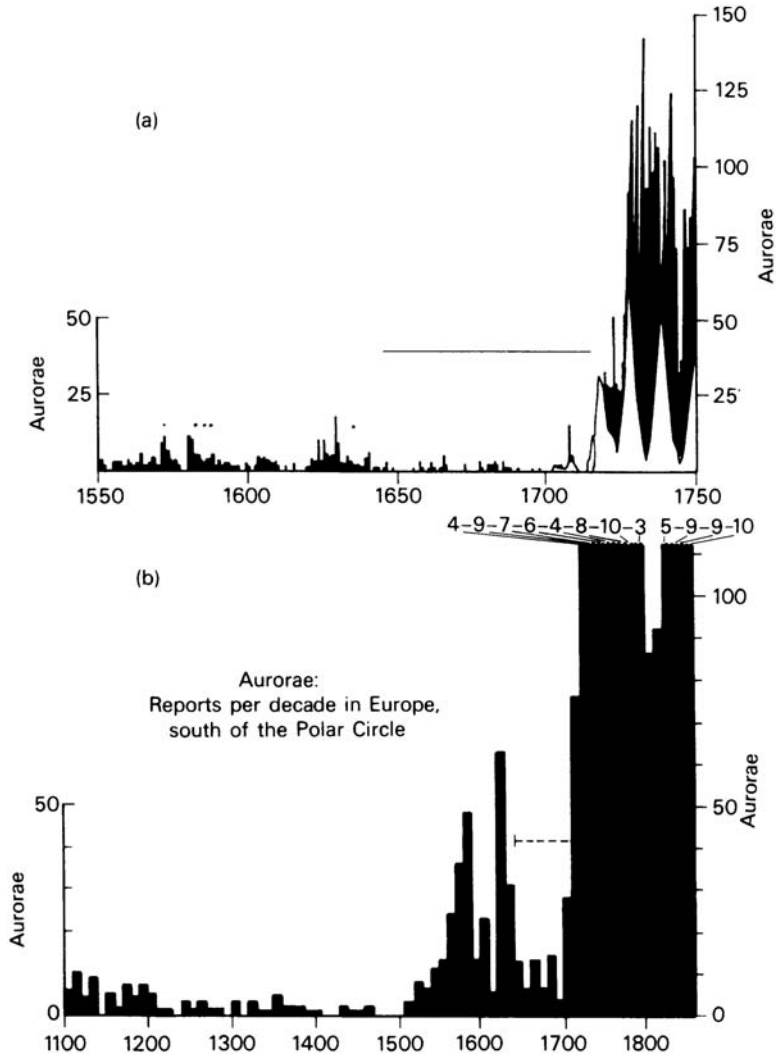

Figure 11.2 Reported aurorae

Source: Fritz 1873

Notes: (a) All reports, from 1550 to 1750 by year, with the annual mean sunspot number superposed as white curves at the right and Far East aurorae (see notes 55, 56, 62) shown as solid squares.

(b) Reports per decade in latitudes 0° to 66°N; counts after 1715 must be multiplied by the numbers shown at the top right of the plot. The period of the 'Maunder Minimum' is shown in each diagram as a horizontal line

The separation of the physical from the sociological in Figure 11.2. is a

question of major importance in studies of the sun and earth. An acceptable solution would involve starting with a new and careful search for auroral data, particularly from northern latitudes, in the New World, Old World and Orient. It must include careful allowance for superstition and vogues and restrictions in observing aurorae, shifts of population, and the possibly important effects of single events, such as the development of the printing press (about 1450), or Gassendi's description of the French aurora of 1621[52] and Halley's paper in 1716.[53] One suspects that the dramatic jump in the number of reported aurorae after 1716 was a direct result of this important paper of Halley, which put the auroral phenomenon on firm scientific footing so that more aurorae were looked for and more regular records were kept.

As for the 'Maunder Minimum', its presence in the auroral record is surely real, appearing in Figure 11.2 as a pronounced pause in the already upwardsweeping curve. Had Maunder looked first at Fritz's auroral atlas, he could have hypothesized a 'prolonged sunspot minimum' from auroral evidence alone.

SUNSPOTS SEEN WITH THE NAKED EYE

Spots on the sun were seen with the naked eye long before the invention of the telescope[54] and were particularly noted in the Far East, where a more continuous record survives. They offer another check on the reality of an extended sunspot minimum, since naked-eye reports of sunspots might be expected were there any strong solar activity at the time. Large spots and large spot groups can be seen with little difficulty when the sun is partially obscured and reddened by smoke or haze, or at sunset or sunrise; small groups or small spots are beyond the effective resolution of the eye and cannot be seen. Thus reports of naked-eye sightings are biased towards times of enhanced solar activity, and attempts have been made to establish the epochs of past maxima in the solar cycle from naked-eye sunspot dates.[55,56]

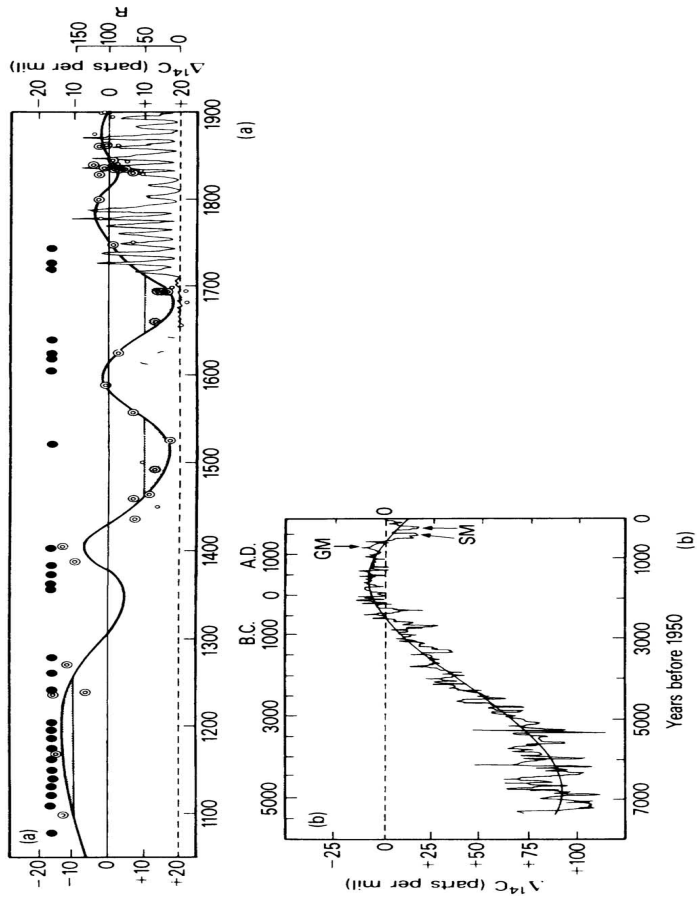

Figure 11.3 (a) History of deviations in the relative atmospheric ^{14}C concentration from tree-ring analyses for the period 1050 mum';to 1900—(see n. 70): single open circles, Northern Hemisphere data; double open circles, Southern Hemisphere data (a heavy line has been drawn through the Southern Hemisphere data); closed circles, dates of reported sunspots seen with the naked eye from Kanda (n. 55). The annual mean sunspot number, R, is shown as a light solid line where known for the period after 1610, from Waldmeier 1961 (n. 3) and this study. Periods when the relative ^{14}C deviation exceeds 10 parts per mil are shaded. They define probable anomalies in the behaviour of the solar cycle : 1100 to 1250, Grand Maximum; 1460 to 1550, 'Spörer Minimum'; 1645 to 1715, 'Maunder Minimum'.

(b) Measured ^{14}C deviation (in parts per mil) since about 5000 BC, with observed (smoothed) curve of sinusoidal variation in the earth's magnetic moment (from n. 80, Figure 2). At about AD 100 the magnetic moment reached a maximum of about 10^{24} gauss per cubic centimetre. Shorter-term ^{14}C excursions attributed in this Chapter to solar cause are marked with arrows: M, 'Maunder Minimum'; S, 'Spörer Minimum'; GM, Grand Maximum in the twelfth to thirteenth centuries. The sharp negative ^{14}C deviation at the modern end of the curve is the Suess effect, due to fossil fuel combustion

Pre-telescopic sunspot observations probably come almost wholly from accidental observation. In Europe reports are rare and fragmentary.[57] It is from the Orient, where sunspots were deemed important in legend and possibly in augury, that we find more extensive and useful records. But here, too, the numbers are small and can only be used as a very coarse indicator of past solar activity.

In 1933 (five years after Maunder's death), Sigeru Kanda of the Tokyo Astronomical Observatory compiled a comprehensive list of 143 sunspot sightings from ancient records of Japan, Korea, and China, covering the period from 28 BC to AD 1743.[58] Most came after the third century, so that the long-term average was about one sighting per decade. Were they distributed regularly (or just at solar maxima), we would thus expect six or seven events during the 'Maunder Minimum'. It is significant that none was recorded between 1639 and 1720—a Far East gap that matches Western Hemisphere data very well.

As with aurorae, the evidence is necessary but not sufficient. Social practices or pressures could have suppressed observation or recording of spots during the time,[59] leading to an apparent but unreal dearth. Moreover, the sunspot gap from 1639 to 1720 is neither the only nor the longest in Kanda's span of reports: there were eighty-four years without any reports of sunspot sightings ending in 1604, 117 years ending in 1520, and 229 years ending in 808 (Figure 11.3a).

We may extend the naked-eye data in a sense by adding dates of reported aurorae in Japan, Korea, and China. All of these lands lie at low auroral latitudes, where displays are expected no more than once in ten years. As in the case of sunspots seen with the naked eye, aurorae reported in the Orient are presumed to sample only intense solar activity. And, as with the sunspot sightings, no Far East aurorae were reported during the 'Maunder Minimum', and more specifically between 1584 and 1770.[60,61,62] The oriental data (sunspots and aurorae) confirm that there were no intense periods of solar activity during the 'Maunder Minimum' and probably no 'normal' maxima in the solar cycle.

We may use the long span of oriental sunspot data as a coarse check on possible earlier occurrences of prolonged sunspot minima, or other gross, long-term modulations of sunspot activity. Of particular note is an intensification of sunspot and aurora reports in the 200-year period centred around 1180, which is about halfway between the 'Maunder Minimum' and a more extended period of absence of Far East sunspots and aurorae in the seventh and early eighth centuries. As I will show below, the naked-eye maximum coincides with a similar maximum of solar activity in the ^{14}C record. If this is a real long-term envelope of solar activity, its period is roughly 1,000 years. We may be measuring only social effects, but, as with historical European aurorae, the subject is one of potential importance which deserves more specific attention by historians.

CARBON-14 AND THE HISTORY OF THE SUN

Modern confirmation for Maunder's 'prolonged sunspot minimum' may be found in recent determinations of the past abundance of terrestrial ^{14}C. Carbon and its radioactive isotopes are abundant constituents of the earth's atmosphere, chiefly as carbon dioxide (CO_2). When CO_2 is assimilated into trees, for example, the carbon isotopes undergo spontaneous disintegration at well-known rates. Thus, by a technique now well established, it is possible to determine the date of life of a carbon-bearing sample, such as wood, by chemical measurement of its present ^{14}C content and comparison with a presumed original amount. The method requires a knowledge of the past abundance of ^{14}C in the atmosphere, and this value is found by analysing, ring by ring, the ^{14}C content of trees of known chronology. The history of relative ^{14}C abundance deviations is now fairly well established and serves as the basis for accurate isotopic dating in archaeology.[63,64,65,66]

The ^{14}C history is useful in its own right as a measure of past solar activity, as has been demonstrated by a number of investigators.[67,68] The isotope is continuously formed in the atmosphere through the action of cosmic rays, which in turn are modulated by solar activity. When the sun is active, some of the incoming galactic cosmic rays are prevented from reaching the earth. At these times, corresponding to maxima in the sunspot cycle, less than the normal amount of ^{14}C is produced in the atmosphere and less is found in tree rings formed then. When the sun is quiet, terrestrial bombardment by galactic cosmic rays increases and the ^{14}C proportion in the atmosphere rises. There are other terms in the ^{14}C equilibrium process, as well as significant lags; but, if there had been a prolonged period of quiet on the sun, we would expect to find evidence of it in tree rings of that era as an abnormally high abundance of ^{14}C.

Such is the case. The first major anomaly found in the early studies of ^{14}C history was a marked and prolonged increase which reached its maximum between about 1650 and 1700,[69] in remarkable agreement in sense and date with the 'Maunder Minimum'. The phenomenon, known in carbon-dating as the DeVries Fluctuation, peaked at about 1690 and is the greatest positive excursion in the recent ^{14}C record—corresponding to a deviation of about 20 parts per mil from the norm. Subsequent studies have established the DeVries Fluctuation as a world-wide effect.

Figure 11.3a shows a curve (open circles and heavy line) of the relative deviation in the ^{14}C concentration based on recent measurements of tree rings,[70] plotted with increasing concentration downward for direct comparison with solar activity; also shown are sunspot numbers[71] (light line) including those from the present work, and years of early naked-eye sunspot sightings from Kanda (closed circles).[72] The three quantities give a wholly consistent representation of the 'Maunder Minimum'. We also note a clustering of naked-eye sunspot sightings at times when the ^{14}C record indicates greater than normal activity, and a general absence of them when the ^{14}C record indicates less than normal activity. Where

annual sunspot numbers are plotted, the ^{14}C curve seems a fair representation of the overall envelope of the sunspot curve. It thus seems valid to interpret the ^{14}C record as an indicator of the long-term trend of solar activity and of real changes in solar behaviour in the distant past, before the time of telescopic examination of the sun.[73,74,75]

We may calibrate the ^{14}C curve for this purpose by noting that the years of the 'Maunder Minimum' define a time when the relative deviation of ^{14}C exceeded 10 parts per mil. If we can make allowance for other effects on ^{14}C production and equilibrium, we may infer that, whenever the ^{14}C deviation exceeded ±10 parts per mil, solar activity was anomalously high or low, with the 'Maunder Minimum' corresponding to a definition of 'anomalous'. We must remember that the ^{14}C indications will tend to lag behind real solar changes by periods of ten to fifty years, because of the finite time of exchange between the atmosphere and trees. By this criterion there have been three possible periods of marked solar anomaly during the last 1,000 years: the 'Maunder Minimum', another minimum in the early sixteenth century, and a period of anomalously high activity in the twelfth and early thirteenth centuries. We can think of these as the grand minima and a grand maximum of the solar cycle, although we cannot judge from these data whether they are cyclic features.

The earlier minimum, which we might call the 'Spörer Minimum', persisted by our 10-parts-per-mil criterion from about 1460 through 1550. Its ^{14}C deviation is not quite as great as that during the 'Maunder Minimum', although that distinction is not a consistent feature of all representations of the ^{14}C history.[76] We can presume that the 'Spörer Minimum' was probably as pronounced as the 'Maunder Minimum' and that during those years there were few sunspots indeed. It appears to have reached its greatest depth in the early sixteenth century when there were also very few aurorae reported.

We noted earlier the possibility of an intensification of solar activity in the twelfth and thirteenth centuries, on the basis of naked-eye sunspot reports from the Orient. Evidence for the same maximum is found in the historical aurora record (Figure 11.2): the number of aurorae in Fritz's catalogue[77] is about constant for the ninth, tenth and eleventh centuries (23, 27 and 21 aurorae per century, respectively), rises abruptly for the twelfth century (53 aurorae), and then falls for the next three centuries (16, 21 and 7 aurorae). The ^{14}C record (Figure 11.3a) shows a similar anomaly in the same direction: a decrease in ^{14}C which could be attributed to a prolonged increase in solar activity.

We must take care in assigning any of the ^{14}C variations to a solar cause, for there are other important mechanisms. The overwhelming long-term effects on ^{14}C production are ponderous changes in the strength of the earth's magnetic field.[78,79] Archaeo-magnetic studies have shown that in the past 10,000 years the earth's magnetic moment has varied in strength by more than a factor of 2, following an apparently sinusoidal envelope with a period of about 9,000 years, on which shorter-term changes are impressed. The terrestrial moment reached maximum strength at about AD 100, at which time we would expect to find a

minimum in ^{14}C production because of enhanced shielding of the earth against cosmic rays.

The good fit of the observed (smoothed) curve of geomagnetic change to the long-term record of fossil ^{14}C is shown in Figure 11.3b, from a recent compilation,[80] here re-plotted with increasing ^{14}C in the downward direction to display increasing solar activity and increasing geomagnetic strength as upward-going effects. Damon[81] has stressed that the long-term trends in the radiocarbon content of the atmosphere have been dominated in the past 8,000 years by the geomagnetic effect, while the shorter-term fluctuations have probably been controlled by changes in solar activity. This point seems clear in Figure 11.3b, where, near the modern end of the curve, the 'Maunder Minimum' *(M)* and 'Spörer Minimum' *(S)* stand out as obvious excursions from the long-term envelope of geomagnetic change. And at about 1200 we find a broad departure in the opposite direction, which might fit the twelfthand thirteenth-century maximum in sunspot and auroral reports. Whether the sun was indeed responsible is open to question, however, for Bucha[82] has pointed out that this ^{14}C decrease follows a similar short-term increase in the earth's magnetic moment (not shown in Figure 11.3b), which had its onset at about AD 900. Moreover, there is uncertainty in the fit of the smoothed archaeo-magnetic curve to the radiocarbon data, and a shift to the right or left will change the apparent contrast of these shorter-term excursions.

We should like to know how solar activity in a possible twelfth-century Grand Maximum compares with the present epoch, but the present is an era of confusion in ^{14}C. The ^{14}C concentration has been falling steeply since the end of the nineteenth century, and the deviation ($\Delta\ ^{14}C$) is now about −25 parts per mil. Were this a solar effect, it would be evidence of anomalously high solar activity. In fact, the sharp drop is an effect of human activity—the result of fossil fuel combustion, which introduces CO_2 with different carbon isotopic abundance ratios—the so-called Suess Effect.[83] If fossil fuel combustion is responsible for all of the modern ^{14}C trend, then during the twelfthcentury Grand Maximum (when industrial pollution was not significant), the natural ^{14}C deviation may have been much greater than at present and the sun may have been more active than we are accustomed to observing in the modern era. There were possibly more spots on more of the sun during the twelfth-century Grand Maximum, and, if the eleven-year cycle operated then, there may have been higher maxima and higher minima than any we see in Figure 11.1. The shallow dip and rise in the fourteenth and early fifteenth centuries (Figure 11.3a) suggest the presence of a subsidiary solar period of about 170 years, but these features seem for now too slight to warrant speculation; we may expect that additional ^{14}C data will clarify the case. The information available at present allows one to describe the history of the sun in the last millennium as follows: a possible Grand Maximum in the twelfth century, a protracted fall to a century-long minimum around 1500, a short rise to 'normal', and then the fall to the shorter, deeper 'Maunder Minimum', after which there has been a steady rise in the envelope of solar activity.[84]

This last phase, which includes all detailed records of the sun and the sunspot cycle, does not appear in the ^{14}C history as very typical of the sun's behaviour in the past, particularly if the *phase* of the long-term curve is important. During most of the last 1,000 years the long-term envelope of solar activity was either higher than at present, or falling, or at grand minima like the 'Maunder Minimum'. As with the present climate, what we think of as normal may be quite unusual. The possibility that solar behaviour since 1715 was unlike that in the past has already been proposed to help explain the sudden auroral turn-on. Another piece of evidence comes from records of the sun's appearance at eclipse.

ABSENCE OF THE CORONA AT ECLIPSE

Historical accounts of the solar corona at total eclipse offer another possible check on anomalies in past solar behaviour. We know that the shape of the corona seen at eclipse varies with solar activity: when the sun has many spots, the corona is made up of numerous long tapered streamers which extend outward like the petals of a flower. As activity wanes, the corona dims and fewer and fewer streamers are seen. At a normal minimum in the solar cycle the corona seen by the naked eye is highly compressed and blank except for long symmetric extensions along its equator. We now believe that coronal streamers are rooted in concentrated magnetic fields on the surface of the sun, which in turn are associated with solar activity and sunspots. As sunspots fade, so do concentrated surface fields and associated coronal structures. Continuous, detailed, observations of the solar corona in X-ray wavelengths from Skylab have confirmed the association of coronal forms with loops and arches in the surface fields and have shown that in areas where there are no concentrated fields, loops, or arches there is no apparent corona.[85]

Were there a total absence of solar activity, we would still expect to observe a dim, uniform glow around the moon at eclipse: the zodiacal light, or false corona, would remain, since it is simply sunlight scattered from dust and other matter in the space between the earth and the sun. At times of normal solar activity the corona seen at eclipse is a mixture of the true corona (or K corona) and the weaker glow of the zodiacal light (or F corona). The latter is a roughly symmetric glow around the sun which falls off in brightness from the limb and is distended in the plane of the planets where interplanetary dust is gravitationally concentrated. If the F corona were ever seen alone, we would expect it to appear as a dull, slightly reddish, eerie ring of light of uniform breadth and without discernible structure.

In fact, first-hand descriptions of total solar eclipses during the 'Maunder Minimum' seem entirely consistent with an absence of the modern structured corona, but proof seems blurred by the customs of observing eclipses in the past and by the fact that scientists seldom describe what is missing or what is not thought to be important. The solar origin of the corona was not established until

the late nineteenth century; before that it seemed equally well explained as sunlight scattered in our own atmosphere, or on the moon. Solar eclipses were regularly and routinely observed throughout the seventeenth century, but not to study the physical sun. They were occasions to test the then popular science of orbit calculation: careful measurement and timing of solar obscuration by the moon offered checks on lunar and terrestrial motions and opportunity to measure the relative sizes of solar and lunar disks. Such details are best obtained not at the eyepiece of a wide-field telescope in the open air but in a darkened room, by projection of the disks of the moon and sun upon a card, as we see in contemporary drawings from the time. Under these restrictive conditions a corona, structured or not, could escape detection, particularly since it appeared so briefly and at just the time when undivided attention was demanded to observe the precise minutia of obscuration.[86]

Nor was it so important to seek out geographic places on the central path of a total eclipse. The corona—K or F—is so faint that it cannot be seen except in exact totality. But if one's purpose were astronomical mensuration and timing, a partial or near-total eclipse was almost as good as a total eclipse and could be observed more accurately in the familiar conditions of permanent observatories. Since partial solar eclipses can be seen over large areas and thus can occur frequently at any location, there was not the impetus of today to travel far and wide to set up camp for one-time tries in distant, hostile lands. Eclipse expeditions are a modern fad that did not take hold until about the nineteenth century.[87]

These fundamental differences severely limit the number of cases we can test. There were sixty-three opportunities to see the sun eclipsed between 1645 and 1715,[88] but only eight of them passed through those parts of Europe where astronomers did their daily work (Figure 11.4). Another case (1698) comes from the New World. Only a few of the European eclipses reached totality near any permanent observatory, and the three best observed occurred at the end of our period of interest—in 1706, 1708 and 1715, when spots had begun their return.

Nevertheless, from this list comes a handful of accounts that bear on the question and answer it consistently. They are descriptions of the corona from the eclipses of 1652, 1698, 1706 and 1708, the only contemporary first-hand descriptions of the sun eclipsed that I can find.[89] They were written, in general, by amateurs and nonconformists who watched the spectacle with eyes open to all of it. None describes the corona as showing structure. Not one mentions the streamers which at every eclipse in the present time are so easily seen with the naked eye to stretch as much as a degree or more above the solar limb. All describe the corona as very limited in extent: typically only 1 to 3 arc minutes above the solar limb. In each case the corona is described as dull or mournful, and often as reddish. No drawings were made. Every account is consistent with our surmise of what the zodiacal light would look like at eclipse, were the true corona really gone.

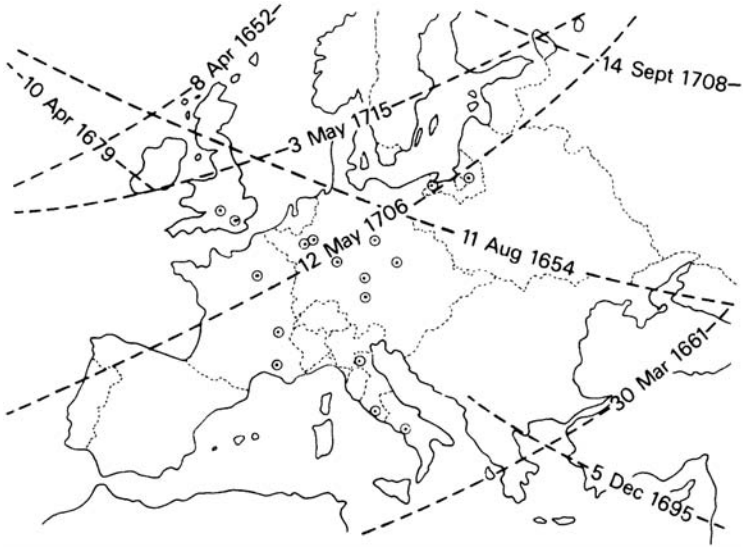

Figure 11.4 Paths of totality for solar eclipses in Europe, from 1640 to 1715. Sites of observatories which reported eclipse observations in the period are shown as double circles

Source: Oppolzer 1962

By 1715 the annual sunspot number had reached twenty-six and was climbing. At the eclipse of that year, at the end of the 'Maunder Minimum', the corona is fairly well described, and for the first time we have drawings of it. For the first time distinct coronal structures are described emanating from the sun. R.Cotes of Cambridge University described the corona (in a letter to Isaac Newton) as a white ring of light around the moon, its densest part extending about 5 arc minutes above the limb; he then added the following:

> Besides this ring, there appeared also rays of a much fainter light in the form of a rectangular cross… The longer and brighter branch of this cross lay very nearly along the ecliptic, the light of the shorter was so weak that I did not constantly see it.[90]

We may presume that the light of the shorter branch was the polar plumes which we see today at times of sunspot minimum and that the longer, brighter branch was the familiar equatorial extensions seen at times of low sunspot activity. Thus by 1715 we find the corona described in modern terms and fitting a familiar form.

In her paper on the dearth of aurorae Clerke mentioned, without example, that it appeared to her probable that during the 'prolonged sunspot minimum' the radiated structure of the solar corona was also 'in abeyance'.[91] In 1973 Parker

repeated Clerke's conjecture.[92] The case for a disappearance of the structured corona during the 'Maunder Minimum' might seem more solid were it not for the fact that the earliest description yet found for the rayed or structured corona at *any* eclipse is that of Cotes in 1715.

R.R.Newton has expressed the situation very explicitly, on the basis of his own researches for definite accounts of the corona as positive documentation of historical solar eclipses:

> The corona is mentioned in most modem discussions of total solar edipses, and to most people it is probably the typical and spectacular sight associated with a total eclipse. In view of this, it is surprising to see how little the corona appears in ancient or medieval accounts….[93]
>
> …there is no clear reference to the corona in any ancient or medieval record that I have found. The most likely reference is perhaps the remark by Plutarch…but the meaning of Plutarch's remark is far from certain.[94]

I should add that here Newton is referring to *any* unambiguous description of the corona, K or F.

A misleading statement common in popular stories of eclipses is that the solar corona was seen in antiquity much as we would describe it today. Usually cited are two early accounts, one by Plutarch (about AD 46 to 120) and another by Philostratus (about AD 170 to 245). Both reports are ambiguous at best, and neither distinguishes between a structured or an unstructured appearance.[95] The situation in all subsequent descriptions before the eighteenth century seems to be no different. At the eclipse of 9 April 1567 Clavius reported seeing 'a narrow ring of light around the Moon' at maximum solar obscuration (although Kepler challenged this as possibly an annular eclipse). Jessenius at a total eclipse in 1598 reported 'a bright light shining around the Moon'. And Kepler himself reported that at the eclipse of 1604:

> The whole body of the Sun was effectually covered for a short time. The surface of the Moon appeared quite black; but around it there shone a brilliant light of a reddish hue and uniform breadth, which occupied a considerable part of the heavens.[96]

None of these or any other descriptions that I can find fit a rayed or structured corona; in many are the words 'of uniform breadth', and it seems to me most likely that we are reading descriptions of the zodiacal light, or of a K corona so weak that its radiance is overpowered by the glow of the F corona.

It could be that, until the scientific enlightenment of the eighteenth century, no one felt moved to describe the impressive structure of the solar corona at eclipse. Indeed, there are other examples from the history of eclipse observation where large and striking features were missed by good observers who were watching other things.[97] Perhaps the rays of the corona at eclipse were thought to be so much

like the common aureole around the sun that they were not deemed worthy of description. Other excuses could be offered. It will be hard for anyone who has seen the corona with the naked eye to accept these explanations and to believe that, of the thousands of observers at hundreds of total eclipses, not one would have commented on a thing so breathtaking and beautiful. It thus seems to me more probable that, through much of the long period of the 'Maunder Minimum' and the 'Spörer Minimum', extending between perhaps 1400 and 1700, the sun was at such a minimum of activity that the K corona was severely thinned or absent altogether. The same may have been true for a much longer span before 1400 and for different reasons may apply as well to the Grand Maximum of the twelfth and thirteenth centuries and possibly earlier. But here the records are so dim and scant that conclusions seem unwarranted. In any case the corona as we know it may well be a modem feature of the sun. It is an interesting question, and another important challenge for historians.

SUMMARY AND CONCLUSIONS

The prolonged absence of sunspots between about 1645 and 1715, which Spörer and Maunder described, is supported by direct accounts in the limited contemporary literature of the day and cited regularly in astronomy works of the ensuing century. We may conclude that the absence was not merely a limitation in observing capability because of the accomplishments in other areas of astronomy in the late seventeenth and early eighteenth centuries, and because drawings of the sun made at the time show almost all the sunspot detail that is known today. Major books by Scheiner and Hevelius, published just before the onset of the Maunder Minimum, describe wholly adequate methods for observing the sun and sunspots. We may assume that a fairly steady watch was kept, since the dearth of spots was recognized at the time and since the identification of a new sunspot was cause for the publication of a paper. We can discount the possibility of seventy years of overcast skies, since there is no evidence of such an anomaly in meteorological lore and since night-time astronomy was vigorous and productive through the same period. Evidence that confirms the 'Maunder Minimum' comes from records of naked-eye sunspot sightings, auroral records, the now-available history of atmospheric ^{14}C, and descriptions of the eclipsed sun at the time.

I can find no facts that contradict the Maunder claim, and much that supports it. In questions of history where only a dim and limited record remains and where we are blocked from making crucial observational tests, the search for possible contradiction seems to me a promising path to truth. I am led to conclude that the 'prolonged sunspot minimum' was a real feature of the recent history of the sun and that it happened much as Maunder first described it.

Earlier in this Chapter I reviewed the possible impact of a real 'Maunder Minimum' on theories of the sun and the solar cycle. For some implications the distinction between no sunspots and a few (annual sunspot numbers of one to

five) is crucial; it is important to know whether during the great depression of the ‘Maunder Minimum’ the solar cycle continued to operate at an almost invisible level, with so few spots that they were lost in our fuzzy definition of ‘zero’. Maunder held that there were enough instances of sunspot sightings through the period to make this case likely, and that the isolated times when a few spots appeared enabled one to identify the crests of a sunken spot curve:

> just as in a deeply inundated country, the loftiest objects will still raise their heads above the flood, and a spire here, a hill, a tower, a tree there, enable one to trace out the configuration of the submerged champaign.[98]

This explanation seems to me unlikely, since the known, visible crests are not at regular spacings. We can hope that more thorough investigation of contemporary literature will enable us to make this important distinction which for now seems beyond the limit of resolution.

The years of the ‘Maunder Minimum’ define a time in the ^{14}C record when the departure from normal isotopic abundance exceeded 10 parts per mil. If we take a ^{14}C deviation of this magnitude as a criterion of major change in solar behaviour, we may deduce from ^{14}C history the existence of at least two other major changes in solar character in the last millennium: a period of prolonged solar quiet like the ‘Maunder Minimum’ between about 1460 and 1550 (which I have called the ‘Spörer Minimum’) and a ‘prolonged sunspot maximum’ between about 1100 and 1250. If the prolonged maximum of the twelfth and thirteenth centuries and the prolonged minima of the sixteenth and seventeenth centuries are extrema of a cycle of solar change, the cycle has a full period of roughly 1,000 years. If this change is periodic, we can speculate that the sun may now be progressing towards a grand maximum which might be reached in the twenty-second or twenty-third centuries. The overall envelope of solar activity has been steadily increasing since the end of the ‘Maunder Minimum’ (Figure 11.5), giving some credence to this view. Moreover, throughout the more limited span during which it has been measured, the solar constant appears to have shown a continuous rising trend which during the period from 1920 to 1952 was about 0.5 per cent per century.[99]

The coincidence of Maunder’s ‘prolonged solar minimum’ with the coldest excursion of the ‘Little Ice Age’ has been noted by many who have looked at the possible relations between the sun and terrestrial climate.[100] A lasting tree-ring anomaly which spans the same period has been cited as evidence of a concurrent drought in the American South-west.[101,102] There is also a nearly 1:1 agreement in sense and time between major excursions in world temperature (as best they are known) and the earlier excursions of the envelope of solar behaviour in the record of ^{14}C, particularly when a ^{14}C time lag is allowed for: the ‘Spörer Minimum’ of the sixteenth century is coincident with the other severe temperature dip of the Little Ice Age, and the Grand Maximum coincides with the ‘medieval Climatic Optimum’ of the eleventh to thirteenth centuries.[103,104]

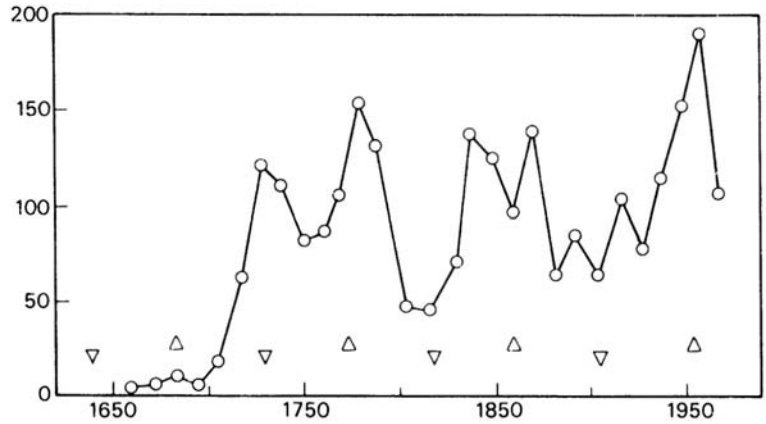

Figure 11.5 Annual mean sunspot numbers at maxima in the eleven-year cycle, from 1645 to 1978, to demonstrate long-term trends in solar activity. Evident is the well-known eighty-year cycle (extrema shown as triangles) imposed on a persistent rise since the 'Maunder Minimum". The seventy-eight or eighty-year cycle was first noted by Wolf, n. 132, and later studied in detail by Gleissberg, n. 133. The solar constant may also have risen slowly through the period during which it has been measured, since about 1908 (see notes 30, 99)

These coincidences suggest a possible relationship between the overall envelope of the curve of solar activity and terrestrial climate in which the eleven-year solar cycle may be effectively filtered out or simply unrelated to the problem. The mechanism of this solar effect on climate may be the simple one of ponderous long-term changes of small amount in the total radiative output of the sun, or solar constant. These long-term drifts in solar radiation may modulate the envelope of the solar cycle through the solar dynamo to produce the observed long-term trends in solar activity The continuity, or phase, of the eleven-year cycle would be independent of this slow, radiative change, but the amplitude could be controlled by it. According to this interpretation, the cyclic coming and going of sunspots would have little effect on the output of solar radiation, or presumably on weather, but the long-term envelope of sunspot activity carries the indelible signature of slow changes in solar radiation which surely affect, our climate.[105]

The existence of the 'Maunder Minimum' and the possibility of earlier fluctuations in solar behaviour of similar magnitude imply that the present cycle of solar activity may be unusual if not transitory. For long periods in the historic past the pattern of solar behaviour may have been completely different from the solar cycle today There is good evidence that within the last millennium the sun has been both considerably less active and probably more active than we have seen it in the last 250 years. These upheavals in solar behaviour may have been accompanied by significant long-term changes in radiative output. And they were almost certainly accompanied by significant changes in the flow of atomic

particles from the sun, with possible terrestrial effects. Our present understanding of the solar wind is that its flow is regulated by closed or open magnetic field configurations on the sun.[106] We can only guess what effect a total absence of activity and of large-scale magnetic structures would have on the behaviour of solar wind flow in the ecliptic plane. One possibility is that, were the sun without extensive coronal structure during the 'Maunder Minimum', the solar wind would have blown steadily and isotropically, and possibly at gale force, since high-speed streams of solar wind are associated with the absence of closed structures in the solar corona. During an intensive maximum, as is suggested for the twelfth and thirteenth centuries, the solar wind was probably consistently weak, steady, and with few recurrent streams.

The reality of the 'Maunder Minimum' and its implications of basic solar change may be but one more defeat in our long and losing battle to keep the sun perfect, or, if not perfect, constant, and if inconstant, regular. Why we think the sun should be any of these when other stars are not is more a question for social than for physical science.

APPENDIX: SUNSPOT NUMBERS

I have used contemporary accounts of telescopic observation of the sun to reconstruct estimated annual mean sunspot numbers for the period from 1610 to 1715 (Table 11.1 and Figure 11.6). Principal sources were Wolf's compilations.[107,108,109,110,111,112,113,114,115,116,117,118] The journal sources are, for the most part, the same as those that were used by Lalande, Spörer and Maunder; thus, except for the direct numerical data from Wolf, Scheiner and Hevelius, sunspot numbers given here are simply a literal quantification of Maunder's descriptive account. Full reliance has been placed on unchallenged statements in contemporary literature which specify periods in which no sunspots were seen, as, for example, between 1656 and 1660, 1661 and 1671, 1689 to 1695, 1695 to 1700, and 1710 to 1713.

Earlier I classified Wolf's historical sunspot data; by the same criteria the data in Table 11.1 should be given a reliability grade of 'poor', since they come from largely discontinuous sets and since allowance for observer and site can only be guessed. The estimated annual sunspot numbers are uncertain to at least a factor of 2, and zero as an annual average means 0 to perhaps 5. The fact that the telescopes of Flamsteed and Cassini were in less than perfect observing sites could have caused these observers to miss a class of tiny, isolated spots which might be detected and counted by keen observers today. The more important point is that their sites and instruments were certainly adequate to detect any level of activity higher than that at the minima of the present solar cycle; they might have missed a few spots but they could not have missed a large number.

My sunspot numbers for the period 1700 to 1715 are somewhat lower than those given for the same period by Waldmeier,[119] who took them from Wolf. Both values are shown in Table 11.1 and Figure 11.6. The general agreement seems

heartening, but the difference may be important since it is in the only span of overlap with other direct numerical compilations. It is also in the least reliable part of Wolf s data and the period of recovery from the 'Maunder Minimum', for which a more gradual rise seems reasonable. Auroral data and eclipse observations from the period of overlap seem to me to support the more suppressed sunspot curve (Figure 11.6). I find it hard to justify Wolf's numbers for his first and possibly second cycles and suspect that his unusualshaped maximum for 1705 was an artificiality of unrealistic correction factors. Wolf did not have confidence in most of the data for 1700 to 1749,[120] and his numbers towards the beginning of that period may represent, more than anything else, a wishful extrapolation of normalcy. I also show in Figure 11.6 and Table 11.1 Schove's estimates of decade-averaged and peak sunspot numbers from the STP,[121,122,123,124] whichwecanalso expect to be systematically high.125

Numbers given for 1625 to 1627 and 1642 to 1644 (from Scheiner and Hevelius) are probably more reliable than any subsequent data in Table 11.1, since they are based on more nearly continuous daily drawings. Data for 1611 to 1613 come from the observations of Galileo. Waldmeier[126] and Schove[127,128,129,130] have apparently followed Wolf in assuming that these three

Table 11.1 Estimated annual mean sunspot numbers, R, from 1610 to 1715; *X*, sunspots noted but not counted; *XX*, unusual number of sunspots noted but not counted; *(X)*, unusually small number of sunspots noted but not counted. Schove's values are for the maxima of each supposed cycle

Year	*R*	*Waldmeier*	*Schove*	*Year*	*R*	*Waldmeier*	*Schove*
1610	X			1663	0		
1611	30	Minimum		1664	0		
1612	53			1665	0		
1613	28			1666	0	Minimum	
1614				1667	0		
1615	X			1668	0		
1616	X	Maximum	90	1669	0		
1617	X			1670	0		
1618	(X)			1671	6		
1619		Minimum		1672	4		
1620				1673	0		
1621	X			1674	2		
1622	X			1675	0	Maximum	60
1623	X			1676	10		
1624	X			1677	2		
1625	41			1678	6		
1626	40	Maximum	100	1679	0		
1627	22			1680	4	Minimum	
1628				1681	2		
1629	(X)			1682	0		
1630				1683	0		
1631				1684	11		

1632	(X)			1685	0	Maximum	50
1633				1686	4		
1634	(X)	Minimum		1687	0		
1635	(X)			1688	5		
1636				1689	4		
1637				1690	0	Minimum	
1638	X			1691	0		
1639	XX			1692	0		
1640		Maximum	70	1693	0	Maximum	30
1641				1694	0		
1642	6			1695	6		
1643	16			1696	0		
1644	15			1697	0		
1645	0	Minimum		1698	0	Minimum	
1646				1699	0		
1647				1700	2	5	
1648				1701	4	11	
1649		Maximum	40	1702	6	16	
1650	0			1703	8	23	
1651	0			1704	9	36	
1652	3			1705	18	58	50
1653	0			1706	15	29	
1654	2			1707	18	20	
1655	1	Minimum		1708	8	10	
1656	2			1709	3	8	
1657	0			1710	2	3	
1658	0			1711	0	0	
1659	0			1712	0	0	
1660	4	Maximum	50	1713	2	2	
1661	4			1714	3	11	
1662	0			1715	10	27	

Source: Waldmeier 1961 and Schove 1955.

islands of data before 1650 sample extrema of the sunspot cycle: Galileo and Scheiner at maxima, Hevelius at minimum. If these periods are all nearer maxima, as I suspect, they give some hint of the fall to the long minimum that followed. The nature of the fall suggests that the telescope was invented barely in time to 'discover' sunspots before their numbers shrank to nearly zero. Had the invention of the telescope been delayed by as little as thirty-five years, the telescopic discovery and more thorough counting of sunspots could have been postponed a full-century, burying forever the principal evidence for the 'Maunder Minimum'.

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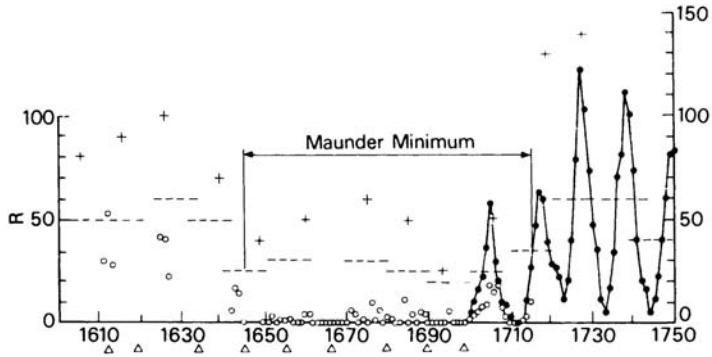

Figure 11.6 Estimated annual mean sunspot numbers, from 1610 to 1750: open circles are data from Table 11.1; connected, closed circles are from Waldmeier, 1961; dashed lines (decade estimates) and crosses (peak estimates) are from Schove, 1947, 1955, 1961 and 1962; triangles are Wolfe's estimated dates of maxima for assumed 11.1–year solar cycle (see notes 3, 7).

E.N.Parker for calling my attention to Maunder's papers, and for personal encouragement in all the work reported here. This research was funded entirely by NASA contract NAS5–3950. The National Center for Atmospheric Research is sponsored by the National Science Foundation.

NOTES

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1 The Wolf sunspot number (or sunspot relative number) is defined as $R = k(10g+f)$ where f is the total number of spots (irrespective of size), g is the number of spot groups, and k is a normalizing factor to bring the counts of different observers, telescopes and sites into agreement.

2 R.B.Leighton, *Astrophys. J.*, CLVI (1969), p. 1.

3 M.Waldmeier, *The Sunspot-Activity in the Years 1610–1960* (Zurich, 1961).

4 R.J.Bray and R.E.Loughhead, *Sunspots* (New York, 1965), p. 1.

5 H.Schwabe, *Astron. Nachr.*, XX (No. 495) (1843). For an interesting discussion of Schwabe, his lonely work, and the prejudice against the idea of cyclic solar behaviour before that time, see M.J.Johnson, *Mem. R.Astron. Soc.*, XXVI (1858), p. 196.

6 See note 3 above.

7 R.Wolf, *Sunspot Observations, 1610–1715*, facsimile of a typescript from Eidgen Sternwarte in Zurich (in G.E.Hale Collection, Hale Observatory Library, Pasadena, Calif.). The eleven-page manuscript lists the days of each year on which spots were or were not seen, the numbers of spots (where known), and notes and references.

Other more condensed accounts of the period by Wolf include: *Astron. Mitt. Zurich,* 1 (1856), p.viii; ibid., XXIV (1868), p. 111.

8 Waldmeier states that ‘Wolf intended to prove for a longer interval the sunspotperiodicity discovered shortly before by… Schwabe’ (1961), p. 8. In one of his papers (*Astron. Mitt. Zurich,* 1 (1856), p.viii, Wolf explained that, in periods where data were sparse, he assumed continued operation of the 11.11–year cycle.

9 D.J.Schove, *Terr. Magn. Atmos. Electr.,* LII (1947), p. 233; *J. Br. Astron. Assoc.,* LXXI (1961), p. 320.

10 D.J.Schove, *J. Geophys. Res.,* LX (1955), p. 127.

11 D.J.Schove, *Ann. N.Y.Acad. Sci.,* XCV (1961), p. 107.

12 D.J.Schove, *J. Br. Astron. Assoc.,* LXXII (1962), p. 30.

13 J.A.Eddy *et al., Sol. Phys.,* XLVI (1976), p. 3; *Science,* CXCVIII (1977), p. 824.

14 F.W.G.Spörer, *Vierteljahrsschr. Astron. Ges. (Liepzig),* XX (1887), p. 323; *Bull Astron.,* VI (1889), p.60.

15 E.W.Maunder, *Mon. Not. R. Astron. Soc.,* L (1890), p. 251.

16 E.W.Maunder, *Knowledge,* XVII (1894), p. 173.

17 E.W.Maunder, *J.Brit. Astron. Assoc.,* XXXII (1922), p. 140.

18 A.M.Clerke, *Knowledge,* XVII (1894), p. 206.

19 See Maunder (1894), p. 173.

20 See Maunder (1894), p. 174.

21 See Maunder (1922), pp. 141–2.

22 See Maunder (1894), p. 174.

23 Late examples include: E.H.Burritt, *The Geography of the Heavens* (New York, 1845), p. 180; R.A.Proctor, *The Sun* (London, 1871), p. 164.

24 W.Herschel, *Philos. Trans. R. Soc. London,* CCLXV (1801). In this wide-ranging and oft-cited paper Herschel reveals his belief in the influence of solar fluctuations on weather, based on his own observation of a correlation between the price of wheat in London and the number of visible sunspots. In making his point, he uses the extreme periods of spot absence of the ‘Maunder Minimum’, during which time the price of wheat rose. Herschel attributes the connection to reduced rainfall when the sun was less spotted, and to the inexorable workings of the law of supply and demand. This paper reveals, among other things, that the quest for a solarweather connection pre-dated the discovery of the solar cycle. It was not Herschel’s worst mistake: in the same paper he tells of his belief in a habitable and possibly inhabited sun.

25 J.LaLande, *Astronomie* (Paris, 1792; and New York, 1966), III, pp. 286–7. This encyclopaedic work was probably the unacknowledged source of most of the nineteenth-century descriptions of past periods of prolonged sunspot absence. LaLande’s references included original journal reports and Jacques Cassini’s *Elements d’Astronomie* (Paris, 1740), pp. 81–2, 182. Jacques Cassini was the son of G.D.(Jean) Cassini, who discovered the sunspot of 1671 and the moons of Saturn.

26 G.E.Hale, ‘Photography of the solar prominences’ (thesis, Massachusetts Institute of Technology, 1890); reprinted in *The Legacy of George Ellery Hale,* H. Wright, J.Warnow, C.Weiner (eds) (Cambridge, Mass., 1972), p. 117; C.A. Young, *The Sun* (New York, 1896), p. 193.

27 R.R.Newton, *Ancient Astronomical Observations and the Acceleration of the Earth and Moon* (Baltimore, 1970), p. 39.

28 R.R.Newton, *Medieval Chronicles and the Rotation of the Earth* (Baltimore, 1972), pp. 99, 600–1.
29 C.M.Smythe and J.A.Eddy, *Nature*, CCLXVI (1977), p. 434.
30 J.A.Eddy, *Bull Am. Astron. Soc.*, VII (1975), p. 365; ibid., p. 410.
31 Attributed to M.J.Rees, in *Project Cyclops*, J.Billingham (ed.) (NASA publication CR 114445, Stanford/NASA Ames Research Center, Moffett Field, Calif., 1973), p. 3.
32 H.C.King, *The History of the Telescope* (Cambridge, Mass., 1955), pp. 50–9.
33 A.H.Pingré (and M.G.Bigourdan), *Annales célestes du dix-septième siècle* (Paris, 1901).
34 This invaluable year-by-year diary of astronomical advance in the seventeenth century was begun by Pingré; in 1756 and completed by Bigourdan in 1901. It illuminates a most interesting century in astronomy and by length alone (639 pages) attests to the vigour of observational work at the time.
35 See, for example, G.D.Cassini, *Anc. Mem.*, X (1688), p. 727; J.Cassini, *Hist. Acad. R. Sci.* (Amsterdam) (1701), pp. 132, 356; ibid. (1702), pp. 185, 194; ibid. (1703), pp. 18, 141,148, 151.
36 C.Scheiner, *Rosa Ursina sive Sol ex Admirando Facularum* (Apud Andream Phaeum Typographum Ducalem, 1630).
37 J.Hevelius, *Selenographia sive Lunae Descripto* (Danzig, 1647).
38 See note 24 above.
39 See Maunder (1922), pp. 143–4.
40 See note 35 above.
41 A.Wilson, *Philos. Trans.*, LXIV (1774), p. 6.
42 The *Rosa Ursina*, although large (25 by 36 by 8 cm) and beautifully set, has not enjoyed kind reviews; comments on the book range from ‘voluminous’, ‘enormous’ and ‘ouvrage considerable renfermant plus de 2000 observations’ to the less couched words of astronomer Jean Delambre: ‘There are few books so diffuse and so void of facts. It contains 784 pages; there is not matter in it for 50 pages’ (*Histoire de l’Astronomie Moderne* (Paris, 1821), vol.1, p. 690); cited in Grant (1852).
43 R.Grant, *History of Physical Astronomy* (London, 1852), p. 216.
44 The ‘Maunder Minimum’ coincided with a prolonged period of distinct climatic anomaly—years of severe winters and abnormal cold—but there is no evidence of unbroken overcast. Astronomers are neither so mute nor so long-suffering that they would have kept quiet through year after year of continuous, frustrating cloud cover. The time was one of vigorous growth and discovery in observational astronomy, as, for example, in the important revelations of Saturn already cited. Throughout the seventy years of the ‘Maunder Minimum’ comets were regularly discovered and observed. We may conclude that during these years skies were at least tolerably clear, and certainly adequate to allow at least sporadic if not normal sampling of aurorae and sunspots, had they been there to see.
45 See Clerke, *Knowledge*, p. 206
46 For this study I have used H.Fritz, *Verzeichniss Beobachter Polarlichter* (Vienna, 1873), which is still probably the most thorough published compilation of ancient aurorae. If criticized, it is more generally for sins of commission than omission; some of the ancient aurorae listed may not have been aurorae at all but meteors or

comets (C.Stormer, *The Polar Aurora* (New York, 1955), p. 14; S.Chapman, in *Aurora and Airglow*, B.M.McCormac (ed.) (New York, 1967), p. 20.

47 Ibid.

48 F.H.Vestine, *Terr. Magn. Atmos. Electr.*, XLIV (1944), p. 77.

49 See note 46.

50 E.Halley, *Philos. Trans. R. Soc. London*, XXIX (1716), p. 406. Halley mentions that the aurora borealis had rarely been seen since the early seventeenth century.

51 Schove (notes 9–12 above) has noted a tendency for auroral counts to alternate by century with more in even centuries (such as the sixteenth and eighteenth) and fewer in odd, in which most of the ‘Maunder Minimum’ took place.

52 See Chapman, *Aurora*, p. 15.

53 See note 50 above.

54 Galileo and the other ‘discoverers’ of sunspots were well aware of the existence of sunspots and naked-eye reports of them before they looked at the sun with telescopes (G.Abetti, in *IV Centenario della Nascita di Galileo Galilei* (Florence, 1966), p. 16).

55 For example, see S.Kanda, *Proc. Imp. Acad. (Tokyo)*, IX (1933), p. 293. Kanda’s compilation is more valuable in its own right than as a clue to past epochs of maxima, since large spots have been known to occur during years of minimum activity.

56 More recent studies of specific ancient oriental sunspot reports have been carried out by D.J.Schove and P.Y.Ho (*J. Br. Astron. Assoc.*, LXIX (1958), p. 295; *J. Am. Orient. Soc.*, LXXXVII (1967), p. 105.)

57 See Bray and Loughead, *Sunspots*.

58 See note 55 above.

59 S.Nakayama, in *A History of Japanese Astronomy* (Cambridge, Mass., 1969), pp. 12–23, has discussed the limitations of the ‘Institutional Framework of Astronomical Learning’ in early Japan and the resultant repression of ideas and research. I have found no evidence that the ‘Maunder Minimum’ was a unique period in this regard, however, and the almost precise coincidence with other evidences from Europe make the Far East sunspot gap seem real to me.

60 See note 55 above.

61 See note 56 above.

62 S.Matsushita, *J. Geophys. Res.*, LXI (1956), p. 297. I have taken from Matsushita’s list only those auroral reports that he deemed ‘certain’ or ‘very probable’.

63 H.E.Suess, *J. Geophys. Res.*, LXX (1965), p. 5937.

64 P.E.Damon, A.Long, D.C.Grey, *J. Geophys. Res.*, LXXI (1966), p. 1055.

65 I.U.Olson (ed.), *Radiocarbon Variations and Absolute Chronology* (Stockholm, 1970).

66 P.E.Damon (personal communication) has compiled radiocarbon data from five laboratories (University of Arizona; State University of Groningen, Netherlands; University of California, San Diego; University of Pennsylvania; and Yale University).

67 M.Stuiver, *J. Geophys. Res.*, LXVI (1961), p. 273; *Science*, CXLIX (1965), p. 533; J. R.Bray, *Science*, CLVI (1967), p. 640; P.E.Damon, *Meteorol. Monogr.*, VIII (1968), p. 151; J.A.Simpson and J.R.Wang, *Astrophys. J.*, CLXI (1970), p. 265.

68 H.E.Suess, *Meteorol. Monogr.*, VIII (1968), p. 146.

69 H.DeVries, *Proc. K.Ned. Akad. Wet. B*, LXI (No. 2) (1958), p. 94.

70 J.C.Lerman, W.G.Nook and J.C.Vogel (in Olson, *Radiocarbon Variations*, p. 275. There are several available compilations of relative ^{14}C concentration; the most commonly cited is probably that of Suess (1968) for Northern Hemisphere trees. P.E.Damon has kindly provided a compilation of ^{14}C data from five world radiocarbon laboratories (see note 66), which has been very helpful in establishing real features. I have used the recent Groningen data cited here since they include a large sampling from trees of the Southern Hemisphere, where the larger ocean surface might be expected to bring about, in effect, faster tree response to real changes in atmospheric concentration. Fluctuations in ^{14}C atmospheric concentration are severely damped out in tree-ring concentrations because of the finite time of exchange between the atmosphere and the trees; the time constant is on the order of ten to fifty years. The presence of absorbing oceans in the equilibrium process acts as an added sink, or leak, and, since the problem is analogous to that of determining changes in the rate of water flow into a bucket by noting its level, a leaky bucket makes a slightly more responsive system. In fact, there are only minor differences between the historical curve of Lerman *et al.* and that given by Suess and others; they show the same extrema at about the same times.
71 See Waldmeier, *Sunspot-Activity*.
72 See note 55.
73 The use of ^{14}C data to deduce solar changes in the past and the possible relation of these changes to the history of the terrestrial climate have been the subject of numerous papers (for example, see notes 67, 68 and 74); J.R.Bray, *Nature (London)*, CCXX (1968), p. 672; P.E.Damon, A.Long and E.J.Wallick, *Earth Planet*. Sci. Lett., XX(1973), p. 300.
74 J.R.Bray, *Science*, CLXXI (1971), p. 1242.
75 P.E.Damon in Olson, *Radiocarbon Variations*, p. 571.
76 The earlier compilations by Suess (n. 63) and by Damon (notes 64 and 66) show that the deviation at 1500 is approximately equal to that of the ‘Maunder Minimum’ period.
77 See note 46.
78 V.Bucha, *Nature (London)*, CCXXIV (1969), p. 681 (in Olson, *Radiocarbon Variations*, p. 501.)
79 R.E.Lingenfelter and R.Ramaty (in Olson, *Radiocarbon Variations*, p. 513.)
80 Y.C.Lin, C.Y.Fan, P.E.Damon and E.J.Wallick, *14th Int. Cosmic Ray Conf.*, III (1975), p. 995.
81 See Olson, *Radiocarbon Variations*, p. 571.
82 See Bucha, *Nature (London)*.
83 See Suess, *Meteorol. Monogr*.
84 See Eddy, note 30.
85 G.S.Vaiana, J.M.Davis, R.Giaconni, A.S.Krieger, J.K.Silk, A.F.Timothy and M.Zombeck, *Astrophys. J.*, CLXXXV (1973), p.L47.
86 The seventeenth-century style of observing solar eclipses is well described throughout Pingré’s compendium (note 33). A principal result from each eclipse was a table giving times of obscuration and the amount of the disk covered in ‘digits’—12 digits corresponding to the solar diameter and total obscuration.
87 A.J.Meadows, *Early Solar Physics* (London, 1970), p. 9.
88 T.R.von Oppolzer, *Canon of Eclipses* (New York, 1962).

89 V.Wing, *Astronomia Instaurata* (London, 1656), pp. 98–102 (note 43, pp. 364, 376–91) (note 33, p. 570); J.Cassini, *Mem. Acad. Sci. (Amsterdam)* (1706), p. 322.

90 Cited in A.C.Ranyard *(Mem:. R.Astron. Soc.,* XLI (1879), p. 503). Cotes might have given a more thorough account had he been free of a perennial eclipse nuisance, for, according to Halley, Cotes 'had the misfortune to be oppressed with too much company' (Grant, *History of Physical Astronomy,* p. 379). Halley's own description of the 1715 corona, from the same reference, follows: 'a few seconds before the sun was all hid, there discovered itself round the moon a luminous ring… perhaps a tenth part of the moon's diameter in breadth. It was of pale whiteness… and concentric with the moon.'

91 See Clerke, *Knowledge.*

92 E.N.Parker, in *Solar Terrestrial Relations,* D.Venkatesan, (ed.) (Calgary, 1973), p. 6; *Sci Am.,* CCXXXIII (September, 1975), p. 42.

93 See Newton, *Medieval Chronicles,* p. 99.

94 Ibid., p. 601.

95 The Plutarch reference is to his account of the solar eclipse of 27 December AD 83; his description follows, as given by R.R.Newton (*Ancient Astronomical Observations* p. 114; *Medieval Chronicles,* pp. 99–100): '[during a solar eclipse] a kind of light is visible about the rim which keeps the shadow from being profound and absolute.' Newton feels that Plutarch's 'kind of light' could be the rim of light visible during an annular eclipse or light from solar prominences, but that, if it is the corona, this is the earliest extant account. In any case it does not help us in answering whether the K corona was seen, since Plutarch's description could as well or better be the zodiacal light. The reference to Flavius Philostratus is from a passage in his fictional and controversial *Life of Apollonius of Tyana,* written about AD 210. Newton avoids it completely, but we should probably expose it to light: 'About the time that [Apollonius] was busy in Greece a remarkable phenomenon was seen in the sky. A crown like a rainbow formed around the sun's disk and partly obscured its light. It was plain to see that the phenomenon portended revolution and the Governor of Greece (the tyrant Domitian] summoned Apollonius…to expound it. "I hear, Apollonius, that you have Science in the supernatural"' [translation of J.S.Phillimore (Oxford, 1912) of book VIII, chap. 23]. In Philostratus' story the 'crown (stephanos) portends the name of Stephanus who later murdered Domitian. The use of the word is thus couched in symbolism and gives no evidence that Philostratus had ever seen either a total solar eclipse or the structured corona.

96 R.Grant (*History,* pp. 377–8) gives the Clavius, Jessenius, and Kepler accounts.

97 J.A.Eddy, *Astron. Astrophys.,* XXXIV (1974), p. 235.

98 See Maunder (1922).

99 E.Opik, *Irish Astron. J.,* VIII (1968), p. 153; see also Eddy, note 30. A change in solar luminosity of 0.5 per cent per century corresponds to 0.005 stellar magnitude per century and is thus outside the limits of practical detection in other G stars.

100 For example, see G.Manley, *Ann. N.Y.Acad. Sci.,* XCV (1961), p. 162; Suess (*Meteorol. Monogr.* n. 68); Bray, *Science* (note 74); *Adv. Ecol. Res.,* VII (1971), p. 177; S.H.Schneider and C.Maas, *Science,* CXC (1975), p. 741.

101 See note 92 above.

102 A.E.Douglass, *Climatic Cycles and Tree Growth* (Washington, DC), I (1919), p. 102; II (1928), pp. 125–6. Douglass found that from 1660 to 1720 the curve of

south-west tree growth 'flattens out in a striking manner' and, before knowing of Maunder's work, he described the end of the seventeenth century as a time of unusually retarded growth in Arizona pines and California sequoias.

103 A good review of past climate history is given in W.L.Gates and Y.Mintz (eds), *Understanding Climate Change* (Washington, DC, 1975) from which the climate incidents cited here were derived. The Little Ice Age lasted roughly from 1430 to 1850 it was marked by two severe extremes of cold, roughly 1450 to 1500 and 1600 to 1700, if we take H.H. Lamb's index of Paris-London Winter Severity as a global indicator.

104 Ibid., appendix A.

105 If changes in the solar constant are reflected in the envelope of solar activity, and if the rate of change has held to the 0.5 per cent per century rate cited earlier (note 99), then we can estimate that during the 'Maunder Minimum' the solar flux was about 1.4 per cent lower than at present—a number not inconsistent with temperature estimates during that coldest period of the Little Ice Age (note 103 above).

106 A.Hundhausen, *Coronal Expansion and the Solar Wind* (Berlin, 1972); A.S. Krieger, A.F.Timothy and E.C.Roelof, *Sol. Phys.*, XXIX (1973), p. 505.

107 See note 7 above.

108 See note 14 above.

109 See Maunder (1890).

110 See Maunder (1894).

111 See Maunder (1922).

112 See note 24 above.

113 See note 25 above.

114 See Pingré and Bigourdan, *Annales*.

115 See note 34 above.

116 See note 35 above.

117 See Scheiner, *Rosa Ursina*.

118 See Hevelius, *Selenographia*.

119 See Waldmeier, *Sunspot-Activity*.

120 See Wolf, *Sunspot Observations*.

121 See note 9 above.

122 See Schove (1955).

123 See Schove (1961).

124 See Schove (1962).

125 The solar emphasis of the Spectrum of Time Project (STP) was first directed at fixing the epochs of presumed eleven-year maxima of the past solar cycle (Schove,1961). Amplitudes of past cycles (ten-year averages) and of past maxima of the cycle were estiinatcd on the basis of the best information available: auroral counts and other unspecified data, with an arbitrary correction for what fraction of aurorae was recorded in a given century (Schove, 1962). Moreover, in the STP there was a built-in constraint to generate nine solar cycles in each 100 years, regardless of whether there was evidence for them or not (Schove, 1955). These and other assumptions tend to dilute possible drastic changes in the past (like the 'Maunder Minimum') and to nullify possible long-term drifts in the amplitude of solar activity. The 'Maunder Minimum' shows up as a significant drop in the number of sunspots in the STP, but with R_{max}=30 at its weakest 'maximum' in 1693, which falls in the middle of a five-year period for which direct accounts from

the contemporary literature report that no spots were seen. It is unfair to press the comparison since the STP covers a much longer span than the ‘Maunder Minimum’ and, more to the point, it should be noted that the STP shows Maunder’s ‘prolonged sunspot minimum’ in Figure 1 and table 2 of Schove (1955).

126 See Waldmeier, *Sunspot-Activity*.
127 See note 9 above.
128 See Schove (1955).
129 See Schove (1961).
130 See Schove (1962).

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